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## EPISTEMOLOGY AND SCIENTIFIC STRATEGY \*

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PHILOSOPHERS of science have become aware of the problems which beset both the scientist and the philosopher when dealing with the meaning attachable to the terms used in high level statements in a physical science. This has been discussed in the past in terms of the syntax and semantics of the theory, the use of reduction sentences by Carnap<sup>1</sup> and Pap,<sup>2</sup> and the pros and cons of a dictionary theory has been discussed by Alexander<sup>3</sup> and Hesse.<sup>4</sup> All of these discussions assume the presence of a well-developed theory. In the biological sciences a similar situation only arises occasionally, as in genetics. For the most part biology is regarded professionally as an observational science and is little worried by the meaning attachable to any high level constructs—because there are so few.

For this reason, the philosophy of science is often regarded by the biologist, who is a practical man—with his hand always touching the laboratory bench—as a study for theorists; this term being used in a derogatory sense. I have been particularly struck by the puzzlement that the practical man expresses when hearing his more philosophically minded colleagues discussing epistemological problems. The practical man is usually not worried by the problems of discovering what the world *really* is.

This paper attempts to show that when a discussion of what the world is really like is brought up, some attempt must be made to come to terms with epistemological assumptions. I hope to show how we

\* Based on a paper read at the second annual conference of the Philosophy of Science Group, Nottingham, 21st September 1957.

<sup>1</sup> R. Carnap, 'Testability and Meaning', *Philosophy of Science*, 1936, 3, No. 4, and 1937, 4, No. 1

<sup>2</sup> A. Pap, *Elements of Analytical Philosophy*, New York, 1949

<sup>3</sup> P. Alexander, this *Journal*, 1958, 9, 29

<sup>4</sup> M. B. Hesse, this *Journal*, 1958, 9, 12

all have implicitly accepted some epistemology, and how a study of these assumptions can help the actual study of specific biological problems. I am going to give a somewhat detailed, though elementary, account of the problems and the rôle epistemology plays in them.

One is frequently told that the essence of science is the making of clear observations and that the act of observing is a way of solving scientific problems. At the same time, those who offer this advice frequently talk of 'doing experiments'. Yet one is struck by the fact that this often involves little more than using complicated pieces of apparatus to make yet further observations. In biology, at the moment, new journals are being produced to be filled with the results of more observations. It is thought that many of these observations will solve hitherto insoluble problems merely because the observer is using an electron microscope. One hears the suggestion 'you need an electron microscope to answer that one'. This emphasis on more and more sophisticated methods of observation leads me to suggest that it might be worthwhile discussing the methodological aspects of the two types of strategy, observation to amass 'facts' and experimentation to test hypotheses, and to consider the epistemological assumptions which both involve.

### I *A Biological Problem: The Golgi Apparatus*

At the end of the last century, in 1898, a histologist, Camillo Golgi,<sup>1</sup> using a silver method which he designed, found that in the nerve cells of the barn owl and the cat a network covered with a deposit of silver could always be demonstrated near the cell nucleus. This apparatus, which he called the 'internal reticular apparatus', has subsequently been called after him—the Golgi apparatus. By 1940 some thousands of papers had been written by other histologists using modifications of the original technique showing slightly different, clearer but reasonably consistent networks in different cells. However, by 1940, phase-contrast microscopy had begun to be used and workers started to look at cells for the Golgi apparatus. Phase-contrast microscopy enabled people to look at living cells, and the results were valued more highly than those obtained from the previous techniques because the latter involved killing the cells, subjecting them to certain chemicals, and therefore the possibility of distortion by the methods of procedure.

<sup>1</sup> C. Golgi, 'Sur la structure des cellules nerveuses', *Arch. Ital. Biol.*, 1898, 30, 60



## EPISTEMOLOGY AND SCIENTIFIC STRATEGY

It was found that there were, in cells seen under the phase-contrast microscope, some refractile bodies, and these bodies, if suitably examined, were found to contain lipoproteins. By using a chemical, sudan black, which reacts specifically with lipoproteins, one worker, Baker,<sup>1</sup> was able to prepare similar material to that used by Golgi and showed that the refractile bodies stained black, but also that although their position in the cell was similar to that of the Golgi apparatus no network was found. Simultaneously other workers<sup>2</sup> in America found that if a piece of dead plant material (pith) was impregnated with lipoprotein and subjected to the Golgi technique the appearance of a Golgi network was found in every dead cell, empty except for impregnated lipoprotein. Baker and his co-workers then described the original network of Golgi as an *artifact* and they coined the word 'lipochondria' to name the sudan-black staining refractile bodies.

Other workers who had done considerable work with the original Golgi technique then looked at their cells under phase-contrast microscopes. They saw the bodies described by Baker, but by considering them to be arranged like a network, described them as a system of canals. Papers by Gatenby<sup>3</sup> and by Dalton and Felix<sup>4</sup> show similar pictures to those of Baker, but canals are described instead of lipochondria and the results were said to confirm the presence of the network as described by Golgi. Lacy<sup>5</sup> more recently came to the same conclusion.

There are now two opposing schools of thought. One school affirms that the network can be seen with the appearance of canals or canal-like systems in living cells and the other says that the network was an artifact and that what were being seen were solid bodies containing lipoprotein. Since the correct description of the structure in the cells can be arrived at only by looking, the matter, we are told, can

<sup>1</sup> J. R. Baker, 'The structure and chemical composition of the Golgi element', *Quart. J. Micr. Sci.*, 1944, **85**, 1

<sup>2</sup> C. E. Palade and A. Claude, 'The nature of the Golgi apparatus. II: Identification of the Golgi apparatus with a complex of myelin figures', *J. Morph.*, 1949, **85**, 71

<sup>3</sup> J. Brönte Gatenby and T. A. A. Moussa, 'The dorsal root ganglion cells of the kitten with sudan dyes and the Zernicke microscope', *J. R. Micr. Soc.*, 1949, **69**, 185

<sup>4</sup> A. J. Dalton and M. D. Felix, 'Studies on the Golgi substance of the epithelial cells of the epididymis and duodenum of the mouse', *Amer. J. Anat.*, 1953, **92**, 277

<sup>5</sup> D. Lacy, 'The Golgi apparatus of the exocrine and endocrine cells of the mouse pancreas', *J. R. Micr. Soc.*, 1954, **73**, 179

be solved by better microscopic techniques involving greater resolution. Several papers<sup>1,2</sup> have now been published which purport to show that using a similar technique to that of Golgi, the cells under the electron microscope show clear evidence of canal-like systems within or associated with the lipochondria described by Baker, and suggest that these canal-like systems could appear as a network as described originally by Golgi.<sup>3</sup>

## 2 Perception and the Golgi Apparatus

I wish to suggest that what appears to be purely a matter of observation really involves a theory of perception and a certain theoretical framework within which perception is operating. The basis of disagreement is best discussed within the theory of perception. The key word is found in Baker's attitude to the original Golgi apparatus. He called it an *artifact*. This word involves epistemological assumptions and it implies that he knows, or has some reason to believe in, some sort of reality. This, as we saw, was partly based on the belief that looking at cells in the living state, or at least without drastic killing, is nearer to looking at reality than by using other procedures. In order to evaluate Baker's claim, I wish to examine the problem as a problem in perception. Moreover, in doing so I feel that any conclusion we come to on the level of observation statements should be equally applicable to constructs found in high level theoretical statements.

(a) *The epistemological situation.* I wish to suggest that the most satisfactory basis on which to erect our epistemology is that of the classical distinction between personal sensible objects—those given to us in sensation, and the world which those sensible objects are *of*—the

<sup>1</sup> D. Lacy and C. E. Challice, 'The structure of the Golgi apparatus in vertebrate cells examined by light and electron microscopy', *Symp. Soc. Exp. Biol.*, 1957, **10**, 134

<sup>2</sup> C. H. U. Chu and C. A. Swinyard, 'Morphological and cytochemical identification of the Golgi apparatus', *J. Biophys. Biochem. Cyt.*, 1956, **2**, 263

<sup>3</sup> I have given an extremely simplified account of the problem. Since 1927 when Walker and Allen claimed that the Golgi apparatus was an artifact which could be imitated by the fixation of lipoid smears there has always been a controversy. For a technical account of the problem and a detailed account of the present position the reader can consult papers by various authors in *J. R. Micr. Soc.*, 1955, **74**, 134-240. One should, however, be warned that the symposium reported in this reference does, in places, give the impression that the problem is purely a semantical one. Careful examination will, I believe, show that this is not the case. Workers are challenging each other's beliefs, not their systems of nomenclature or classification.



world of physical objects.<sup>1</sup> I realise that this distinction is philosophically unfashionable and raises difficulties of its own in terms of causation of the sensible object world and so forth, but for our purpose I think the distinction is both useful and adequate. I also want to suggest that because the sensible object world is personal to each of us, when we want to communicate we find it necessary to *invent*, as an epistemological hypothesis, the world of physical objects just in order to discuss our experiences with other people. In the simplest cases, using only intuitive assumptions, we can discuss the hypothetical world of physical objects. With these intuitive assumptions we communicate on the basis of our private sensible-object-worlds sufficiently well so that usually we are in little difficulty. When I talk about perceptual beliefs, I shall be referring to beliefs about our hypothetical world of physical objects. It is worthwhile noting that we never know when our beliefs about this world are true. In fact, I disagree with those who believe that the truth of what we have invented is important. Rather, I believe that the falsification of our beliefs is more important, because by falsification we can change any statements about the physical object world that do not give us a basis on which to act reliably in the future. Thus the writings of some philosophers of science on the status of existential hypotheses and their meaning misses the point, for, I believe, the existence or otherwise of entities described by such hypotheses does not matter. What we are (or should be) concerned with are the logical relations of such hypotheses. Our language system is first and foremost a calculus. We can derive consequences from the higher level hypotheses and so long as the lower level statements we have derived can be statements about sensible objects, we shall be able to predict what other people who share our language will be aware of and invite them to test the whole calculus by their experience.

At the observation level in ordinary speech we do not distinguish between sensible-object-language and physical-object-language, but an examination of what is involved in perception suggests that to do so would sometimes be useful in science. Thus, with the Golgi controversy what is happening is that perceptual beliefs of both sides

<sup>1</sup> A more explicit account of the sense in which I am using these terms is given in J. H. Woodger, *Physics, Psychology and Medicine*, London, 1956. Woodger considers in Chapter 7 a four-termed relation:  $x$  gets (a view, feel, taste, etc.)  $y$  of  $z$  under circumstance  $w$ . The second domain of this relation (i.e. the  $y$ 's) are what I call sensible objects, while the third domain (i.e. the  $z$ 's) are what I call physical objects.

about physical objects are being confused with beliefs about sensible objects. We must notice that some interpretative statements which are involved in the relation **of** (the relation between sensible and physical objects) must enter into the expression of a perceptual belief. This is because it is on the basis of agreement about interpretative statements that personal sensible objects can lead to agreement about shareable physical objects and communication between observers can be carried out. Something must be done about the basis of agreement of the interpretative statements so that A can tell B what sort of sensible object he should receive of the physical objects under discussion in a given set of circumstances.

(b) *Application to the Golgi controversy.* The difficulty of separating the two aspects of our perceptual beliefs can be seen when we realise that the two sides in the Golgi controversy both use similar techniques and yet one side perceives refractile bodies while the other side perceives canals and networks. The sensible objects known by each side we can assume to be similar. What they are disagreeing about are the physical objects which their sensible objects are **of**. The side which feels stronger then calls the perceptual beliefs about the physical objects of the other—artifacts. The word unfortunately lays emphasis on disagreement about the production of different sensible objects from physical objects about which there appears to be agreement, whereas in fact it is the physical objects themselves about which there is disagreement.

Seen in this light the problem reduces itself to deciding which sort of physical object is the best for explaining the sensible objects personal to both sides. The trouble in the Golgi controversy is that except for incidental theoretical concepts such as the chemical reactions involved in staining lipoproteins, there is at the moment no conceptual framework for deciding between the various types of personal belief. This conceptual framework is involved in testing *any* personal belief. When we make a so-called simple observation, we only make it because there is some implicit conceptual framework into which it fits. I would go so far as to say that the observation is not merely made on this conceptual framework alone, but is very frequently made during the *falsification* of the conceptual framework. An example, it may be classic, is that of noticing the presence of a clock in a room when it has stopped, because the conceptual framework involves a set of expectations which were falsified by the lack of sound which was part of the expectation. This is a very simple set of



conceptual beliefs. But consideration of other examples of the act of observing and the 'invention' of the physical world shows that they *always* involve some act of interpretation and therefore some framework within which this can be done.

(c) *Other interpretative statements.* Let us briefly examine other types of observation statement found in science to see how the interpretative statements enter.

(1) First consider those met in the historical sciences. It was shown by Gould<sup>1</sup> that statements about the past, as he called them, are made on the basis of two other types of statement. Gould showed that the statement 'this is a fossil plant' is derivable from two types of statement:

- (i) 'this is a lump of black material' which is a statement about the present involving a physical object and says something about—in terms of colour—the sensible object of it.
- (ii) Laws about present day plants and their behaviour when they are subjected to considerable pressure and when they become infiltrated with carbon to become black objects. Also involved are laws about sedimentation rates of different types of material, and morphological laws.

On the basis of these laws we conclude that this here-and-now physical object is a certain fossil plant of such and such an age. Statements of type (ii) Gould called interpretative statements.

(2) Another example can be taken from psychology where interpretative statements could generate two distinct types of perceptual belief. Consider an interviewing situation. The interviewer can describe a person's behaviour in terms of the various sensible objects that he had of this person plus the physical object spatio-temporal relationships that go with them. For example, for him to say 'Tom is angry' is to use the word 'angry' as a name for a collection of sensible object-physical object Toms under certain conditions. The interviewer will be *getting* certain views of Tom under certain specific circumstances. Such views under such circumstances he can call views of 'angry Tom' and this can serve to define 'angry Tom'. This definition could be given to many interviewers and agreement can be reached on the sort of view all could expect and all might get. This would lead to an objective psychology. On the other hand, the interviewer could also mean by 'Tom is angry' that the impact of

<sup>1</sup> R. P. Gould, this *Journal*, 1957, 8, 192

the sensible object Tom on himself was such as to produce the feeling which generates the awareness in the interviewer that if he were behaving as Tom was behaving *he* would *feel* angry. This would lead to a subjective type of psychology. The problem would then be how to get agreements over statements involving the second type of percept as a firm basis for psychological theorising.

### 3 *The Rôle of Experiment*

To return to the problem we posed in connection with the Golgi controversy: how does one distinguish, in science, between our various perceptual beliefs of the physical object world? First let us remember that the reason for inventing this world was (a) to communicate and (b) to live with other people in a world where prediction helps our behaviour. Therefore we find that the physical object world as expressed in scientific statements is not constructed in a haphazard fashion. We are interested in making statements about the *relationships* between physical objects and particularly in those relationships which, because they are invariant, enable us to predict the behaviour of that world in the future. Science is sometimes said to be the *search* for invariant relationships. I would rather say that it was the testing of statements *about* invariant relationships. However, in either case the first step in science is to construct general statements involving relationships between physical objects. These general statements are of two sorts.

(1) They can be generalisations of the relationships on the basis of which all we can do is to associate physical objects in the future simply as instances of the generalisation. Such a statement I would call a *descriptive* statement and I want to call the type of prediction we make from them a *forecast*. An example of this type of statement would be the statement that 'Dogfish have hearts on their ventral sides'. This enables us to forecast the position of the heart in any given dogfish in the future. I suspect that many of the so-called laws in the physical sciences are descriptive statements of this sort. For example, if I wanted to forecast the position of the hands of a clock at some time in the future I can do so by working out the relative angular movement of both hands until I arrive at a general statement from which I could make my forecast.

(2) However, there is another type of statement relating physical objects, which frequently occurs as a higher level statement and when



## EPISTEMOLOGY AND SCIENTIFIC STRATEGY

it occurs as a low level statement encourages the invention of higher level ones. These statements relate physical objects causally. I do not want to be drawn into a discussion of the implications of causality and a simple example will show what I have in mind. Consider the statement:

‘Stimulation of the vagus nerve slows down the heart beat.’

This involves the description of spatial relations between physical objects, but, by including the temporal relation, gives rise to a *descriptive* statement of a *process*. It has the merit of provoking us to ask how the process comes to be and, as a result of the provocation we, or someone else, invents another process statement which is *explanatory* of the first. In this way higher level statements are made. From the higher level statements not only can the statement about the stimulation of the vagus be derived and from it a forecast made, but more than this. In so far as the higher level statements usually involve mentioning new physical objects in new spatio-temporal relations with one another, they will also enable us to derive new statements about *these* entities. Where it is possible to suggest what sensible objects will be *of* these physical objects we shall be able to make what I shall regard as a prediction proper. It will suggest new observations about new things, all of which will in part be already explained.

To go back to my clock example. I can forecast the position of the hands at some future time by reference to statements about cogs, balance wheels, and springs. I could also predict the position of some of the cogs relative to the balance wheels and so forth. What I have done is to invent entities whose invented relationships to one another have enabled me to calculate the position of the hands. I can only test these causal statements by deriving statements from them and then interpreting these statements into *sensible* object language. The process of testing these causal laws is the process of experimentation—and it is the fundamental process in scientific strategy.

The situation then is that in the case of Golgi controversy we are faced with the problem of how to decide on the satisfactoriness of physical objects for the interpretation of the sensible objects seen by both sides and in the case of explanatory hypotheses, how to test causal laws involving physical objects which explain causal laws at a lower level. Both sides face the same problem: how can we test our invented physical world which is primarily concerned with causal relationships? The answer here is by experiment and *not* by observation alone.

What is involved in experimentation? The first characteristic of an experiment is that it involves testing some hypothesis. Moreover, the only type of hypothesis which we *can* test by an experiment is one involving causal laws. I am making this point in this way because although many so-called experiments involve apparatus, etc., they really are nothing more than observations, and we have seen that observation involves a theoretical framework within which the observation is made. Mere observation therefore begs the question of distinguishing between the processes in which physical objects are interacting, and as we have seen this is our purpose in testing the physical object world.

A second characteristic of experiments is involved in the idea that science is based on a strategy founded on the belief that it is possible to 'interfere' in the physical object world. You may ask how, if I have *invented* a physical object world, can I interfere within it? I believe the answer is that strategy involves two things:

- (a) We can look, listen, sense, etc.
- (b) We can interfere.<sup>1</sup>

The only way of knowing about either of these activities is by carrying them out. The object of the interference is to alter the conditions in such a way as to attempt to falsify some causal law involving the physical objects. This then is the second characteristic of experiments. By altering the conditions I do what is called a control experiment. This involves selecting the physical objects in such a way that they represent a sample from the total population of all such physical objects. It is presumably what Bronowski means when he says that critical experiments are highly stratified samples in the variables under discussion.<sup>2</sup>

How can such an experiment tell us something about physical objects? Let us look briefly at enzyme histochemistry. We want to know something about the site of enzymes in cells.

Any histochemical procedure is based on the following theory. If a cell component has a known chemical structure, then by thinking

<sup>1</sup> To speak of looking at, listening to, etc. or of interfering with physical objects, which are at the same time regarded as hypothetical, may cause difficulty; for how can we listen to, or interfere with, something that in principle may be a fiction? The difficulty is circumvented by avoiding using 'listening to' or 'interfering with' in an object language and by speaking about them instead in a metalanguage.

<sup>2</sup> J. Bronowski, 'The Logic of Experiment,' *The Advancement of Science*, 1952, 9, 289-326.



out a reaction which this chemical should undergo with a dye, the presence of the chemical in the cell can be detected. The procedure using the dye emphasises the need to *visualise* (a technical term) the cell component in some way. The reaction is discovered by the usual biochemical methods of taking bulk tissue in test-tubes, breaking it down, and seeing first whether the reaction is possible in this environment. In the case of an enzyme such a reaction could involve making a compound in which a dye is attached to a grouping which could be separated off by the enzyme. The compound is usually colourless and in one such case,<sup>1</sup> when the group which is broken off by enzyme action is removed, the colourless compound left is immediately oxidised to a coloured compound—in the case which I am considering this is blue indigo. By attempting to set up conditions so that the oxidation of the colourless dye to blue indigo is practically instantaneous, problems such as the diffusion of dye from the original site of its supposed breakdown are eliminated, and therefore the site in the cell at which the dye is formed is said to be the site of enzyme activity in that cell, and one is said to be able to observe the site of enzyme activity in that cell. The situation is slightly more complicated than this but I do not wish to go into biochemical details. I merely want to give the example as an instance of a visualisation process.

At the observational level we deal with the solutions in test-tubes in terms of sensible objects. In order to explain different appearances we invent chemical reactions involving, in some complexity, theoretical constructs—ions, valency bonds, molecules, and so on. In order to explain why in some cases reactions are accelerated, we invent enzymes. All these constructs refer to physical objects. We can define them in terms of accelerated reaction rates, but also, in some cases, we can define enzymes as crystalline substances. It is only when we have this kind of ostensive definition that we get to the level of our physical object enzyme having a sensible object of it. Up to this point the physical object enzyme was only introduced as a notion to explain the other objects we have sensed.

When we demonstrate the presence of an enzyme in a cell we can be regarded as doing two things.

(1) We are testing causal laws about chemical systems and predicting that when some physical objects react in a certain way

<sup>1</sup> S. J. Holt and R. F. J. Withers, 'Studies in enzyme histochemistry, V. An appraisal of indigogenic reactions for esterase localization', *Proc. Roy. Soc. B*, 1958 148, 520

another physical object will be produced, and we are able to test these laws because we know what sort of a sensible object should be produced in the end—in our case, a blue dye.

(2) We can also be regarded as observing where in the cell the enzyme is, and this is the sort of question for which histochemistry is usually used.

A moment's reflection will show that the second sense is conditional on the first, so that as an observation of the physical site of physical-object enzymes in a physical-object cell it cannot stand on its own as an observation. Moreover, since a microscope is invariably used to look at the cell, the theory of the optics of compound microscopy also comes into the system of theoretical knowledge.

Thus, even in an apparently observational problem such as enzyme histochemistry, the basic process is not one of observing but of 'interfering' with the chemical reactions in such a way as to test all our hypotheses about these reactions *and* the optics of the microscope, *and* yet at the same time to produce an expected sensible object in the end.

#### 4 *An Application of the Methodology to a Typical Problem*

A problem can be envisaged in the study of the mechanism of the regulation of carbon dioxide in the blood of vertebrates. The regulation is connected with a part of the organism called the carotid body, in which there are believed to be special cells which are receptive to certain chemical changes in the blood. The problem would be to find which cells of the carotid body might be the chemoreceptors. The carotid body could be studied by looking for cells in it which show copious innervation under the ordinary microscope. This has been done, and some cells are found called the glomus cells. However, in order to make *sure* that these are the chemoreceptors, the typical approach is to make more observations. It could be suggested that to study the glomus cells under the electron microscope would solve the problem. Here attitudes might differ. One worker might believe that under the electron microscope he will be able to see the mechanism of the glomus cells. Another, perhaps with more understanding, might feel that he must do some experiments involving changes in the carbon dioxide concentration in the blood round the glomus cells, and he might express the hope that under the electron microscope he will see some differences within the glomus cells under differing conditions of carbon dioxide concentration; but—and I want to suggest



that this would be a slight limitation in his outlook—he would not know what to expect.

My suggestion would be that, instead of complicating the theoretical system within which the workers propose to find their physical objects by using an even more complicated theoretical framework within which to do it (electron microscopy), it would be more profitable to *attempt to find some causal laws* in which hypothetical cell components might be related to variation in carbon dioxide concentrations in the blood. In the testing of these causal laws they would both make observations in the second sense described above, and at the same time be attempting to falsify their hypotheses—but their theory must involve the deduction of sensible-object statements so that they know what to look for. For example, if their theory involved the movements of ions across a cell membrane and nothing more, they would know that they cannot observe ions even under an electron microscope. It would therefore be necessary to extend the theory so as to include, say, a possible pH change within the cells which, under specifiable conditions, might give rise to, say, an increase in glycogen content in the cells. Then, with a histochemical technique, they could produce sensible objects of the glycogen in the cell. If they carried out some such procedure they would not even need electron microscopy.

Similarly, a causal hypothesis is needed in the Golgi controversy as to what a physical Golgi apparatus is doing in the cell. It would only be by a choice based on experiments—in terms of predicting new sensible objects—that we would have any method for deciding which of the two possible physical-object Golgi apparatuses was the most satisfactory explanation of the original appearances.

## 5 Conclusion

I believe that experiment is the only way in which we can test our knowledge of the physical object world. Observation alone never provides us with a method for choosing what sort of physical objects we need to invent. We invent physical objects because we want them to take part in causal laws from which we will be able to predict. It is not easy to invent the causal laws, but neither is it necessary to assume that they will arrive like magic from observation. Genetics is the most advanced of the biological sciences because it has a system of causal laws about genes which lead to satisfactory predictions. But

R. F. J. WITHERS

we must remember that so far we have no sensible object of a gene. The fact that the invention of causal laws is difficult is no reason why we should not try to find them.

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## ORIGIN EXPLANATIONS AND THE ORIGIN OF LIFE\*

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THE perennial question of the origin of life has been opened up for a new round of discussion. Several articles<sup>1</sup> have appeared within the past four years, differing in detail and emphasis, but sharing a common purpose, to bring this chronically difficult subject within the scope of scientific explanation. The authors of these articles share a confidence that recent discoveries in biophysical chemistry now put us in a position to offer—in outline at least—a scientifically satisfactory explanation of the origin of life.

In reading these articles we came to feel somewhat disoriented; we developed a complaint whose source was not clear. In discussing it together, we agreed that we had no objections to the geological, astronomical, biophysical, or biochemical facts or theories presented. On most of this neither writer feels competent to judge, and we are happy to be informed. Neither have we complaints that the offered explanations of the origin of life are incomplete or sketchy—for none of the articles makes any pretence at completeness. It became apparent that the source of our complaint was with the style of the explanations offered—not with the stuff—with certain concepts and the way these were used in the explanations. In short the disorientation was philosophical, logical. This article is an attempt to get at the sources of this disorientation.

\* Received 18. vii. 58

<sup>1</sup> J. D. Bernal, 'The Origin of Life', *New Biology*, 1954, No. 16, 28; H. F. Blum, 'Perspectives in Evolution', *American Scientist*, 1955, 43, 595; Melvin Calvin, 'Chemical Evolution and the Origin of Life', *American Scientist*, 1956, 44, 248; J. B. S. Haldane, 'The Origins of Life', *New Biology*, 1954, No. 16, 9; H. Jacobson, 'Information, Reproduction and the "Origin of Life"', *American Scientist*, 1955, 43, 119; N. W. Pirie, 'The Meaninglessness of the Terms Life and Living', *Perspectives in Biochemistry*, London, 1937; N. W. Pirie, 'On Making and Recognizing Life', *New Biology*, 1954, No. 16, 41; J. W. S. Pringle, 'The Evolution of Living Matter', *New Biology*, 1954, No. 16, 54; George Wald, 'The Origin of Life', *The Physics and Chemistry of Life*, New York, 1955.

At times we felt that the form of the explanations was disarmingly—and confusingly—simple. Explaining the origin of life is not logically the same kind of enterprise as explaining the origin of the custom of shaking hands. Nor is it the same sort of thing as explaining the origin of species. It shares some logical features with each. We try in this article to get clear about the logical character of an explanation of the origin of life by comparing and contrasting it with other kinds of origin explanations. In doing this, we think we can bring out the source of a confusing feature of these recent discussions, viz. a mixture of abstract biochemical theory with seeming concern about the nature of some particular living things.

At other times, in reading these articles, we felt that we were being given too much: that in addition to an explanation of the origin of life, some other extraneous things were added, and that these additions were not necessary to a satisfactory explanation. Indeed, they detracted from it. Some of these extraneous ingredients were 'the nature of life', 'primal life', 'the first cell'. In 1937, N. W. Pirie warned against definitions of 'life'. We accept his warning as good methodology on this point. But we try to show here how very very tempting it is when giving an explanation of the origin of life, to indulge in something, which if not giving a definition of life, is very much like giving one.

We need not, we feel, speak of explanations—in the plural—of the origin of life. From the standpoint of our present enquiry the explanations have the same form. Hence, we shall speak of all the authors cited as presenting *an* explanation of the origin of life. The offered explanation seems to consist of the following elements (with differences in detail noted):

- (1) Organic compounds are known to be produced out of inorganic by non-organic processes.<sup>1</sup>
- (2) Once formed they would survive, because
  - (a) at the right time the atmosphere of the earth had little or no oxygen.<sup>2</sup>
  - (b) there were no organisms able to destroy them.<sup>3</sup>

<sup>1</sup> Bernal, op. cit.; Blum, op. cit.; Calvin, op. cit.; Pirie, *Perspect. in Biochem.*; Pirie, *New Biology*.

<sup>2</sup> Bernal, op. cit.; Calvin, op. cit.; Haldane, op. cit.; Pringle, op. cit.; Wald, op. cit.

<sup>3</sup> Calvin, op. cit.; Wald, op. cit.



## ORIGIN EXPLANATIONS AND THE ORIGIN OF LIFE

- (c) certain forces are present which promote their formation. There is a spontaneous formation of aggregates of different types (processes of biochemical selection begin).<sup>1</sup>
- (d) structures utilise surrounding organic molecules, first by:
  - (i) fermentation, using energy-rich phosphate bonds.<sup>2</sup>Then by:
  - (ii) photosynthesis which produces oxygen,<sup>3</sup> and so:
  - (iii) respiration.<sup>4</sup>

(A) Suppose someone were to say, 'When walking along the street with women, men customarily walk on the outside. What is the origin of this custom?' How would one answer? If he knew a few facts about history there should be no difficulty. He could say, for example, 'In the Middle Ages, refuse was thrown from house windows. Someone walking close to the building was less likely to be splattered with it than someone walking on the outside. Hence, men wishing to protect the dignity and finery of their women, took to walking always on the outside. It became an item of chivalry, and men who did not observe the courtesy were criticised.'

(B) Suppose next that a child has been studying the early history of the United States, and a lesson ends with, 'and we can still learn a lot about our history by studying the customs of New England'. What if the pupil now asks, 'How do *customs* originate?' This is a bit more difficult and one might be hesitant to give an answer, but for purposes of our discussion try this: 'At a certain time people do things for what is, or is believed to be a good reason. These things become sanctioned, and violators are criticised, ostracised, or even killed. Because of the conservative nature of man in his society, these modes of behaviour are preserved even after the reasons no longer apply and are never again given, after no sanctions remain except the reminder, "That's just the way it's done", etc.'

Each of these answers to questions about origins seems to be a perfectly satisfactory type of origin explanation. They are very different in many respects; some of the relevant differences will be brought out presently. Since we are going to use these explanations as landmarks

<sup>1</sup> Bernal, op. cit.; Blum, op. cit.; Calvin, op. cit.; Pringle, op. cit.; Wald, op. cit.

<sup>2</sup> Blum, op. cit.; Calvin, op. cit.; Haldane, op. cit.; Wald, op. cit.

<sup>3</sup> Bernal, op. cit.; Haldane, op. cit.; Wald, op. cit.

<sup>4</sup> Haldane, op. cit.; Wald, op. cit.

or reference places, it is important to note that they are kinds of explanations about which we could have no radical misgivings. They answer the questions; they do their jobs; they explain in familiar ways. Any disagreements with them would be disagreements in detail, not objections in principle. We would know—if we only knew enough of the facts—what disagreements to bring against the explanations, and how to settle the matter.

There is an obvious and close relationship between the two. The second (B)<sup>1</sup> gives the plan or pattern which the first explanation (A) follows. When making the first explanation it is tacitly understood that there is such a plan to follow. Although one might follow or use it, he still might not be able to state it. Stating it is difficult for several reasons, among these, that it makes explicit certain things which go unmentioned in the first explanation, viz. human beings being what they are, certain modes of behaviour will continue when no reason can be given for them. We shall refer to this element in (B) by saying that explanations of this type state a 'principle of persistence' which is presupposed by any explanation of the first (A), type. Note that the principle is not *assumed* by one who makes an explanation of the first type (he knows perfectly well that human beings have the required conservative character); it is just not mentioned. The first explanation is the proper answer to a certain question. In order to answer that question, a person does not have to answer some other related question. It would be quite mistaken to say that the answer to the first question was incomplete.

Anyone who gives the first explanation (A) or who accepts it as an explanation of the appropriate type is bound to accept the second one (B)—at least in principle. One can disagree with (B) in only two ways, in principle, or in detail: (1) He might insist there are human customs which are *explained* in an entirely different way, or (2) he might insist that there are human customs whose origin is not in reasoned action. The first objection would be to the effect that the plan could not (would not?) be applied as broadly as was stated. In collecting evidence to support this objection, one would need to survey human explaining behaviour, as much as the actions involved in the customs explained. The second objection is to the effect that the plan followed, in so far as following gives an explanation, is not properly

<sup>1</sup> We shall follow the practice of using a letter to stand for the explanation given as example at the beginning of the lettered section. Thus, '(A)' is an abbreviation for 'the example explanation given in A', etc.



stated. The proposed statement of plan mentions an element extraneous to explaining the origin of a custom. In short, it is not the correct statement of the master plan which it pretends to be.

When (B) is looked at by contrast with (A), it seems appropriate to say that (B) does not primarily give information about history or human actions or customs. It does not primarily give information at all. Rather it gives guidance in how to make *A*-type explanations—which do give information. Secondly, however, (B) does give information about customs and origins, for it does have the subsidiary force of a generalisation to the effect that all human customs do have certain features in common. The subsidiary rôle of this generalisation is shown by noting that the features which all customs are asserted to have in common are only those, specific instances of which would be mentioned in an *A*-type explanation. Had the child's question been raised in other circumstances, an answer in the same words would have made the generalisation more prominent. Suppose the pupil had just been given the first origin explanation (A), and then asked, 'How do *customs* originate?' Then the same words used in (B) would have had more prominently the force of 'All *customs* begin with some reasoned action. . . '.

In the original context such an explanation contains a generalisation about all customs but only in so far as they are matter for explanation by a certain pattern. It certainly does not contain a generalisation about explanations, to the effect that every proper origin explanation of a human custom always has and always will be of such and such a form. We cannot generalise to the plan-explanation by looking over a random assortment of instances of particular explanations, because the relevant instances all presuppose and apply it. The main question about (B) it seems is whether we have got properly stated what we have all along been using in explanations such as (A).

The point we are trying to make is this: the first type (A) explains the origin of a specific custom. The second (B) has conspicuous among its functions the statement of a plan which when followed by the first makes it an acceptable type of explanation. Note that it is natural to ask questions calling for specific origin explanations with either question 'What is the origin of . . . ?' or 'How does . . . originate?' but it is more natural to ask for the plan by 'How do . . . s originate?'. Compare: 'What is the origin of the earth?' with 'How do planets originate?'

With these two landmarks laid down, (A) and (B), we think it can

already be seen that answers to questions about the origin of life are more like the second (B) than the first (A) kind of explanation. Are those who write on the origin of life concerned with any specific living form? This seems to be the source of some confusion. It is at least clear that they are not answering the question 'How do living forms originate?'. That was Darwin's question. Perhaps before looking at the origin of life question, it would be helpful to consider it. We wonder if a little mischief has been worked by Darwin's phrase 'origin of species', so that we tend to phrase his question, 'What is the origin of species?' rather than 'How do species originate?' To bring out the character of Darwin's question, let's consider first the explanation of the origin of a specific living form.

(C) Consider the question 'What is the origin of man?' It has been reasonably well answered in outline. Fossil evidence seems to demonstrate that there were forms very early in geological time that closely represent a common ancestor of apes and man. These forms have anatomical structures common to both groups, yet lack many of the unique characteristics which are features of each. On the basis of such evidence, man and apes appear to have followed separate lines of evolution from a monkey-like quadruped (or ape-like quadruped) as far back as the Oligocene or Miocene epochs of time.

(D) Now compare this with Darwin's question, 'How do species originate?' A satisfactory summary of Darwin's answer might go like this. There is random variation among the progeny of any living thing, and these variations are inherited. The environment exacts a heavy toll, killing a large number of living things before they can reproduce. Those best suited to the environment survive; hence the variations of survival-value are passed on to future generations. Given long periods of time these changes from generation to generation, add up to a new species.

It is quite clear that Darwin's purpose was to work out a new plan, the plan for a 'scientific' explanation as opposed to mythological or theological ones. He did not get it stated quite right; we have since learned that there are very great difficulties with any plan as simple as Darwin's. We are not here trying to get it stated adequately; we want to point out that in explaining the origin of man, we presupposed some such Darwinian plan and applied it. The relation between Darwin's answer and the explanation of the origin of man is parallel to the plan for customs (B) and its application to a special custom (A). Also, the Darwinian plan plainly states what is pre-



## ORIGIN EXPLANATIONS AND THE ORIGIN OF LIFE

supposed in the explanation of the origin of man, a principle of persistence, via inheritance. This plays a rôle analogous to the conservative nature of societies in the explanation of the origin of customs. In the Darwinian plan another principle is also mentioned: random variation and survival. We shall refer to this type of principle in an origin-plan explanation as a 'principle of complication'. No such principle entered into the statement of the origin-plan for customs explanation—presumably because the required principle is universally understood, and not called into question.

Now what does one do when he gives an explanation of the origin of life? Mostly plan-giving, it would seem. All recent writers are in agreement, apparently, that what is needed are principles of complication by which organic compounds could arise from non-organic ones, amino acids from simpler compounds and elements, proteins from amino acids, etc., etc., and principles by which these could be expected to persist once formed: tendency toward liquid crystallisation, large size molecules, energy-capturing transformations such as the use of energy-rich phosphate molecules. It is agreed by all that some of these principles are now understood. There is no concern with what specific crystalline structures may have been floating on the surface of the sea four billions of years ago. We are not yet ready for the minute, precise details. We do not know which details to look for until we have some plan to apply. The present problem would seem to be using what is known about biophysical chemistry to construct the plan.

Yet there is more than that. Whereas these recent articles are not concerned with specific chemical combinations, they are concerned in general with chemical conditions on the earth four billions of years ago. They are reporting on conditions which led to the emergence of life much as one reports on the refuse pots and muddy streets at a certain time in history in explaining the origin of the male-outside custom. Is the concern with past chemical conditions merely the concern over whether the emerging plan will be applicable? If one of the likely principles of complication can be applied only to inorganic compounds in an oxygen-less atmosphere, it is important to know whether the atmosphere at the time this complication process might have been going on was of the right kind.

Still, there is a greater note of specificity than this in these articles. At times they seem to be talking about some specific life-form, or some definite event in pre-history.

As these properties accumulated in some time in the past, they would have reached a degree of development in some single system of which had we been there viewing it from the outside, we might have said, 'that, now, is living'.<sup>1</sup>

. . . since the origin of life belongs to the category of at-least-once phenomena, time is on its side. However improbable we regard this event, or any of the steps which it involves, given time enough it will almost certainly happen at least once. . . .<sup>2,3</sup>

(1) They are right to be concerned (to some extent, at least) with prehistoric particulars. The landmarks we have set up so far are misleading. There are many species and many customs, so one expects to follow a common pattern in explaining the origin of each of them. As far as we presently know, there is only one 'life' and accounting for its origin is explaining the origin of a special thing. Yet one cannot explain a special thing by using a pattern which fits no other thing. The pattern is one for biochemical complications of all kinds which produce living things among other complex products. Then the plan supplied by these recent writings on the origin of life is not an answer to the question 'How does life originate?', but an answer to the question 'How do complex protein-systems originate?'. Therefore, much (in fact most) of these articles is not concerned with the origin of life but with the plan to be used in explaining the origin of life. We can answer the questions 'What is the origin of  $x_1$ ,  $x_2$ ,  $x_3$ , etc.?' only when we can appeal to the plan which is given in the answer to 'How do  $x$ 's originate?'

Then these articles attempt to do two things: (1) state a new plan for explaining the origin of biophysical and chemical complexes, and (2) apply the plan in giving an explanation of one of these complexes, life. The rubric followed by these articles is this: they collect together certain recent discoveries in biophysical chemistry and bring them to bear on an explanation of the origin of life. When we place this against the landmarks so far discussed, one feels inclined to say it contains a hidden complexity: a mixture of plan-stating, and plan-applying going on reciprocally. Nothing is wrong with that. Certainly not; but we hope that putting it this way may help to reveal why these authors also get concerned with other disturbing enterprises.

When we attempt to give an account of the origin of life in combination with the drawing up of a novel, incompletely formulated plan for the origin of protein systems, we are naturally led to wonder

<sup>1</sup> Calvin, *op. cit.*

<sup>2</sup> Wald, *op. cit.*

<sup>3</sup> See quotations on pp. 112, 115



## ORIGIN EXPLANATIONS AND THE ORIGIN OF LIFE

how the plan fits. We must ask, 'What sort of protein system is a living thing?' In order to bring out the nature of this question, consider another example.

(E) Suppose a quarry-worker says to a visiting geochemist, 'See these egg-shaped limestone nodes sticking out of the sandstone cliffs. How did they originate?' The geochemist answers something like this. 'A substance, calcium bicarbonate, which is soluble in water, is found widely distributed in the earth. When water percolates through sand or sandstone, it often carries this substance in solution. In pockets or openings where evaporation takes place, it accumulates and undergoes a chemical change into solid, insoluble calcium carbonate.' Here is an origin-explanation for which the plan is a simple, well-understood, chemical transformation; it is part of a broad, theoretical framework from which similar plans can be taken by the dozen. The plan is presupposed and applied. The particular explanation assumes that the listener is a theoretically sophisticated person. Suppose, however, that he is not, and that he replies, 'I can see that you have accounted for something—I believe you called it calcium-carbonate—coming to be in these holes. But how about this limestone?'

There is no simple answer to this. If the geochemist is patient, he begins lessons in chemistry. However, before the lessons begin, by way of insisting that his explanation was a good one, he might say, 'Calcium carbonate *is* limestone'. In order to explain how chemistry will give the questioner an understanding of the origin of limestone formations, he might add, 'If you could see the substance resulting from the changes I am talking about, you would recognise it as limestone'. Of course he is not giving a definition of the word 'limestone', nor discussing how the novice recognises limestone when he sees it. The quarry-worker undoubtedly knows how to use the word 'limestone', and the geochemist is certainly not trying to disturb his linguistic habits, nor to express doubts about his mastery of the word.<sup>1</sup>

The geochemist, while giving an origin explanation of limestone, cannot possibly have the question, 'Is calcium-carbonate limestone?' It is a layman's question. However, the question, 'What sort of protein-complex is life?' is not just a layman's question. Here the

<sup>1</sup> We realise that statements like 'Calcium carbonate is limestone', i.e. abbreviated expressions of bridges between theoretical and everyday concepts, often have an almost irresistible metaphysical 'pull'. We do not think it necessary to go into this difficult business here.

plan is not presupposed as part of some simple, fairly fixed theory, nor is its application a routine matter. To make clear the application of the plan, an answer is required. One could not pretend to give an explanation of the origin of life unless he could give an answer. True, the answer might be vague, but that would be in keeping with the degree of finish and completeness of the plan, which is admittedly sketchy.

Note the style of some of the answers:

The critical event which may best be called the origin of life was the enclosure of several different self-reproducing polymers within a semi-permeable membrane.<sup>1</sup>

If some mechanism maintains the supply of trypsinogen there is 'growth' of trypsin, but this is not looked on as a sufficiently organised activity to make the system qualify as living. If, however, the system were able to make the trypsinogen as well, when supplied with amino acids and sugar or with acid derivatives, it would be difficult to defend the exclusion logically.<sup>2</sup>

. . . Leading finally to the emergence of localised reaction centres which merit the title of organism.<sup>3</sup>

Why do we have here the phrases 'may best be called', and 'difficult to defend the exclusion logically', and 'merit the title of'? Suppose the geochemist had said 'Calcium carbonate may best be called limestone', or 'It would be difficult to defend not applying to calcium carbonate the word "limestone"'. Perhaps we can bring out the difficulty here by comparing the origin of life explanation with some other landmarks. One of these is chosen because it is a notoriously unsatisfactory origin-explanation.

(F) Take the question, 'What is the origin of Christianity?' A very incomplete answer would run something like this. Jesus of Nazareth, in his early life, was a member of a monastic community which taught purity of life, and studied the writings of the prophets with fervour. In particular the community interpreted certain prophetic writings as predicting the coming of a holy man-saviour. Jesus, at some time, broke with the community and went about teaching its moral code to the masses. He became convinced that he was the predicted saviour, and was put to death because of this religious presumption. Some of his disciples came to believe that he was God, and that his life and death were actions of God in bringing about the salvation of believers from sin and death.

<sup>1</sup> Haldane, *op. cit.*

<sup>2</sup> Pirie, *New Biology*

<sup>3</sup> Pringle, *op. cit.*



## ORIGIN EXPLANATIONS AND THE ORIGIN OF LIFE

(G) Now, how about the plan question 'How do religions originate?' A likely answer would refer to a leader of unusual powers who brought about reforms in the existing religion or religions of a people and after whose death was believed to be divine or a possessor of divine powers. It is clear that this sort of explanation of how religions originate is related to the explanation of the origin of Christianity as plan to application, i.e. as (B) was to (A). But now we have in this area a third origin question.

The question 'What is the origin of religion?' has been much with us. This question is not a request for an explanation of the origin of any specific religion or group of religions (like F), nor is it a request for a plan or programme for explaining the origin of each and every religion (like G). It is like the origin of life question in being a request for a pre-historical story. It is unlike in that it presupposes a plan which is so well understood that there is rarely need for it to be stated. The question has been given many answers: none of them has been accepted by many at any time, or by any for long. Speculative answers have been so wild, that present-day anthropologists have generally thrown the question out of court—for the reason that we have no significant information on the required pre-history and are not likely to get any. Consequently the question can only provoke uncontrolled speculation. Anthropologists have seldom seen a more fundamental defect in answers to the question.

(H) Consider a typical (though out-of-date) answer. Primitive men believed that when they dreamed they were actually doing what they dreamed they were doing—while, of course, they were sleeping. They believed a 'second self' had left their sleeping bodies and had done what had been dreamed. By a slight extension they came to believe that this 'second self' left the body at death and never returned. So they imagined that the ghosts of the dead were somewhere around and operative. When an important leader died, a leader upon whom the society was greatly dependent, they came to believe that his powers and his counsel were still available, if techniques could only be found to use them. Hence, ritual, prayer, and a professional class of those specialised in making available the powers of the departed leader. As time goes on dependence upon these invisible powers increases—and they come to be considered powers of some superhuman agency.

Of course, this is a crude account, but it is difficult to see how fussing with the details would make it more satisfactory. It is difficult to see how anything of the kind could give a satisfactory explanation. We

know what the great world-religions are, and we know something about the origins of each of them. If we assume that we have already given in (G) something like the correct plan of explanation for the origin of a religion, then we are prepared in principle to understand how one religion can emerge from another. What an explanation of the origin of religion (H) pretends to give is an account of how a religion grows out of non-religion. The above proffered explanation (H) seems deficient precisely because that whose origin is explained is not clearly identifiable as a religion.

This defect has generally been felt by writers in speculative anthropology, and they have tried to complete their explanations in two ways: (1) By adding some such statement as: seeking assistance from the super-natural powers of a departed leader is the first in a series of more and more complex activities which ends with religion as we know it today. This manoeuvre is a barely disguised admission that no explanation of the origin of religion has been given. A gap is felt between that for which an origin has presumably been given and that for which an origin is wanted. A pretence is made at bridging the gap by a series of 'and-so-forths'. This is obviously a bogus form of supplement.

Compare this with the way Calvin explains what sort of protein-system a living thing is. He discusses how an auto-catalytic system with a phosphate linkage might originate. Then, instead of telling what sort of system a living thing is, he writes:

Finally, as the systems evolve in complexity, at some period of time they may acquire all of the collection of qualities that are usually attributed to living things, and we can say the thing is alive, or that there is a living system present.<sup>1</sup>

Note also that Wald, after discussing the possibility of natural formation of colloidal aggregates, says:

We suppose that all these forces and factors, and others perhaps yet to be revealed, together give us eventually the first living organism.<sup>2</sup>

(2) Speculators on the origin of religion have more frequently added to their account another bogus supplement which runs something like this: Religion is basically (or essentially, or primarily) an effort to get assistance from imaginary powers. Unlike 'Calcium carbonate is limestone' this is meant to bridge a gap felt by the theorist as well as the layman-learner. An explanation of the origin

<sup>1</sup> Calvin, *op. cit.*

<sup>2</sup> Wald, *op. cit.*



of religion is offered. It takes the form of explaining in some detail the origin of  $x$ . But now how about the origin of religion? The answer is, ' $x$  is religion, the very essence of it'. Unlike 'calcium carbonate is limestone' this answer insinuates that without special tutoring we cannot be depended upon to correctly identify religious behaviour when we are confronted with it. It casts doubts upon the correctness of our non-theoretical talk about religious rituals, religious organisations, and religious doctrines. It disturbs our confidence in our mastery of certain regularly used parts of the English vocabulary. But we can and do read about religious conferences and religious leaders. We distinguish one religion from another, and we know something about the origin and distinctive features of each. We realise also that religious behaviour, literature, etc., shades off into the non-religious. We are aware that many things people say or do are neither clearly religious nor non-religious. All of this does not make it impossible to explain the origin of religion, nor does it lead us to want a definition of 'religion'.

We do not know what to do with one when it is offered. We are puzzled as to how it fits into any scheme which explains the origin of religion. We also know that some things people do are a result of forethought, some things are reasonably based habits, some customs. These shade into one another, but it does not follow that one needs to know the essence of 'custom' in order to explain the origin of one.

What rôle do such statements as these play in an explanation of the origin of life?

[a living organism is] . . . the site of a continuous influx and outflow of matter and energy. This is the very sign of life, its cessation the best evidence of death.<sup>1</sup>

Life, then, is a temporary reversal of a universal trend by means of the production of information mechanisms.<sup>2</sup>

The style of such statements strongly suggests that the authors feel a conceptual gap in their explanations. They attempt to close it by telling what life is. Compare this with the other gap-closing devices. Gaps were closed in (*E*) by telling what limestone is, and in (*H*) by telling what religion is. But the gaps in (*E*) and (*H*) are not of the same kind. (*E*) is a two-level origin explanation. The origin is discussed in the technical vocabulary, and involves the conceptual apparatus, of a branch of one of the sciences. This second-level explanation

<sup>1</sup> Wald, op. cit.

<sup>2</sup> Jacobson, op. cit.

is then applied in answering a first-level question, i.e. a question involving only plain, every-day concepts, and stated in untechnical language. A gap existed only in the layman's lack of understanding as to how the technical and un-technical were to be connected. An indication of how they were in fact connected could be made by coming straight down from a technical conception to a layman's conception by means of the formula 'Calcium carbonate is limestone'. The origin of religion is a one-level affair. The words used in the explanation are understandable without tutoring in symbolically expressed equations, geometrical models, or in the use of special instruments. The road, a straight and easy road, ends before we get to our destination. Instead of extending the road, an attempt is made to move the destination closer by word magic; but it cannot be done.

The origin of life is a two-level explanation. It runs up and down between the conceptions of everyday affairs and those of the biochemist; but in the end, that whose origin is explained is an entity understood only by the biochemist. All of the authors seem cognisant of a gap. It is not clear whether this is because they are writing for laymen; or whether, because of the novelty of the plan, they wish to make its application more than usually plain. In any case their language is disturbing. Some come directly down from the conception of a protein system to life, but they do so with such phrases as 'may best be called life', or 'merits the title of life'. Others descend from their biochemical talk not directly to life, but to certain traits which could be exhibited by a protein-system whose origin they have explained, an exchange of energy and matter with the outside. They have returned to the common road, but they seem to feel—and quite correctly—that they are not at their destination. But do they finish the journey? Some seem just to promise that they will arrive. Others seem to turn to word magic, and tell us that life really is just a matter of exchange of energy with the environment. Part of the trouble seems to be that it is forgotten how vague a word 'life' is. If one thinks there must be a sharp line between the living and non-living, it is tempting to locate the line so as to just include the system whose origin has been explained.

A picture of a sharp line between the living and the non-living may also explain the disturbing particularity of these stories of the origin of life. It is almost at times as if an attempt were made in applying the plan, to translate it totally into a one-level account, tracing a straight line development from inorganic compounds to the living organism.



## ORIGIN EXPLANATIONS AND THE ORIGIN OF LIFE

It sometimes seems as if we are told a prehistoric story of a sequence of systems, each more complex than the one before, until at last, the very first living thing has appeared. The idea of the sharp line between the living and non-living could encourage this picture, but it will not account for it. We need to make a connection between some concept used in the plan of biochemical complication, and the concept of a living thing; but there is another reason which tempts us to make that connection with the first, simplest living thing. The plan for biochemical complication is meant to complement Darwin's plan. Darwin's plan (without the later supplementation of genetic theory) is a one-level explanation. In explaining the origin of any species, it calls for tracing its ancestry to a preceding species, then to a preceding one, etc. There is an inclination to conclude that there must be one species to which Darwin's plan does not apply, viz. the very first. It is the origin of this which needs to be explained; this is the goal of the theory of biochemical complication. The applied story of biochemical development is the story which leads up to the first form of life. We easily became possessed by a picture-understanding of Darwinian theory: diverging lines which branch off from one trunk line which itself begins at some definite point.

Of course this picture is misleading. One cannot even describe it without the flavour of caricature. No one would think of finding and reporting records of some one species, the very first species, to which the Darwinian plan did not apply. Compare the problem of the origin of life with that of the origin of a custom. Like living things, customs change in time. Since its origin a given custom has probably changed considerably. We have principles to explain these changes, and they are not the same principles used to explain origins. With customs, however, we have no tendency to think that some one performance, some one certain movement was the very first enactment of the custom. We have no temptation to ask 'Which man's walk, and on what date, and at what hour and place was the first outside-walk the performance of a custom?' We know this is a joker-question like 'Which hair was it, when lost, that made him bald?' or 'Which brick made the load too heavy, and broke the springs?'

Asking about the first form of life is the same sort of question. Even so, there is a great temptation to think that when explaining the origin of life, one is explaining the emergence of the first living thing. And we suspect that some of our writers have yielded to this temptation.

For the primal organism, generated under the conditions we have described, . . .<sup>1</sup>

One is asking, in effect, for an apparatus which is the unique property of cells in order to form the first cell.<sup>2</sup>

It is between this stage and the first recognizable organism that the largest gap still exists. . . .<sup>3</sup>

Long particles . . . , spherical particles, . . . Both may have occurred in the first formation of preorganismal life.<sup>4</sup>

Aggregates . . . interact . . . to form larger and more complex structures. In this way we imagine the ascent, not by jumps or master strokes, but gradually piecemeal, to the first living organisms.<sup>5</sup>

Nevertheless, it is very difficult to dispel the picture of the explained appearance of the first living organism, especially when it is so strongly abetted by other features of the explanation.

We think that we have pointed out what some of these features might be. In the first place the origin of life cannot be a simple narrative like the story of the origin of Christian baptism. There is no well-understood plan, no known principles of complication and persistence which it can apply. So, the explanation must state its own plan. Secondly, the plan is a selection from and a quasi-narrative assembly of, principles of biochemistry; its statement involves the models, technical terminology, and abstruse symbolism of one of the more abstract sciences. But an explanation of the origin of life requires a plan narrative; it must be something like an explanation of the origin of a custom, or an institution, or a geological formation. The explanation of the origin of life cannot be just a second-level plan statement; it must also be a plan application in the form of a first level story of the past.

Admittedly the requisite knowledge of pre-history is lacking. The application can at best be made at a few critical past epochs; it must be sketchy and speculative. Since the explanation positively requires a pre-historical story, there may well be a temptation to give the speculative suggestions a sound of particularity which is misleading. Regardless of how partially or completely the plan can be applied at certain points in the pre-historical account, it *must* not fail of application to the end-product of the developmental story, the living thing. Two kinds of gaps may have to be bridged: (1) the conceptual, from protein-systems to living things or to something simpler than living things, and (2) the pre-historical, from some simpler-than-living system to the

<sup>1</sup> Wald

<sup>2</sup> Ibid.

<sup>3</sup> Bernal, op. cit.

<sup>4</sup> Ibid.

<sup>5</sup> Wald, op. cit.



## ORIGIN EXPLANATIONS AND THE ORIGIN OF LIFE

living thing or at least to the kind of thing to which Darwinian principles apply. At these points temptations might arise to bridge the gaps by odd formulae like, ' $x$  may best be called life', or by promises and etceteras. Or again it may seem possible to close the gap by properly plumbing the essence or nature of life. There may be temptations to complete the explanation with incantations.

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# SOURCES OF SCEPTICISM IN ATOMIC THEORY\*

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## I

I WANT to distinguish two types of assertions, doubts, denials, concerning the existence of atoms and other like entities. One type I shall call the scientific assertion, doubt, etc., or more generally doubt on the phenomenological level ( $\phi$ -level).<sup>1</sup> The other I shall call an epistemological assertion, doubt, etc. ( $e$ -level).

To take  $\phi$ -level discourse first. At this level, it will make straightforward sense to say such things as that, whilst originally we had very little evidence for the atomicity of matter, later discoveries greatly strengthened our belief in the physical reality of atoms. Or again, the following way of talking will be in order: Whilst originally the assumption of chemical atoms seemed plausible, subsequent discoveries threw considerable doubt on this. (I shall in a moment discuss this case in detail.) Even this will be perfectly in order: some features of the atoms of elementary kinetic theory are clearly contrary to physical possibility (e.g. zero-volume); to this extent they are abstractions or fictions. In all these cases strictly scientific considerations will determine the language of our choice, a language which operates within a conventional framework where expressions such as fiction, physical hypothesis, bare assumption, evidence for the existence of, and so on, have a relatively fixed grammar. Similarly, the usual distinctions between direct and indirect evidence will be made without causing any questions. Thus if we say that we have only indirect evidence for the existence of atoms, we shall of course mean that we have direct evidence for something else and that all this is a matter of degree; for instance, that we 'see directly' the instruments which give us information about these atoms.

\* The substance of this paper was given at the Third Conference on Philosophy of Science, Oxford, 21st September 1958.

<sup>1</sup> Phenomenology. The science of phenomena as distinct from that of being (ontology). The term is here meant to refer to discourse at the commonsense realist and scientific level, accepting the categories with which it operates.



## SOURCES OF SCEPTICISM IN ATOMIC THEORY

Now compare this with epistemological discourse. Its outstanding feature is that it is very general and knows few limits. It does not usually make the sort of distinctions I have just mentioned. Mach is a good instance of this approach. Discussing the kinetic theory of matter, he tells us that there is no doubt that

natural phenomena really do exist that act *as if* the pressure and impact of very small particles were involved in their production. . . .<sup>1</sup>

Now the reason for this view is not just the tentative nature of kinetic theory during the sixties of the nineteenth century. It is that

every (!) physical notion is nothing more than a definite connection of the sensory *elements*. . . . These elements . . . are the most ultimate building stones of the physical world that we have as yet been able to seize.<sup>2</sup>

Such notions, like the law of refraction, caloric, electricity, lightwaves, molecules, atoms, and energy *all* and in the same way ' must be regarded as mere helps or expedients to facilitate our view of things '.<sup>3</sup>

To take a second example, the Ramsey-Braithwaite view has the same sort of generality. Here the inverse-deductive model provides its fascination. When I say a theoretical entity exists, very roughly I can mean no more than that from supposedly true propositions employing the corresponding theoretical concepts other propositions can be deduced that are testable by observation.<sup>4</sup> The generality of this argument emerges a few pages later when Braithwaite uses the same logical schema to explain the meaning of general statements. For, so we are told,<sup>5</sup> the same (!) problem arises for both the word ' every ' as for the word ' electron '. And just as for the case of Mach, so here also the meaning of general statements draws its substance solely from the logical consequences which are deducible from them when taken together with some particular premiss. One can see what a radical sense of indirectness that is!

Let  $H(x)$  stand for an hypothesis containing a theoretical term  $x$ , deductively yielding an experimentally verifiable proposition  $E$ . The inductive logician often describes the situation by saying, not that  $H(x)$  is true, but that  $H(x)$  is supported by  $E$ . And so far, the question of  $x$ 's existence would not even seem to come up, whatever the actual existential status of  $x$  may ultimately turn out to be. It is easy to

<sup>1</sup> E. Mach, *Monist*, 1892, 2, 199

<sup>2</sup> E. Mach, *op. cit.* p. 205

<sup>3</sup> *Ibid.*, p. 202

<sup>4</sup> R. B. Braithwaite, *Scientific Explanation*, Cambridge, 1953, p. 79

<sup>5</sup> *Ibid.*, p. 82

express this by saying that we do not know whether  $x$  exists but only that  $H(x)$  leads to verifiable consequences, and more radically, that it is absurd even to speak of the possibility of  $x$ 's existence.

Now such a position would land us once again in the muddling of  $\phi$ -level and  $e$ -level discourse. First, let us note that the inverse-deductive model by itself does not *entail* the meaninglessness of the question of the existence of  $x$ , and this for two reasons: (a) One may maintain that the proposition  $H(x)$  makes (or implies) an existential assertion,<sup>1</sup> though it does not of course entail its truth. (b) The inverse-deductive model is quite neutral to the existential status of  $x$ . Thus, the entity designated by  $x$  *may* be physically isolable (e.g. bacteria) or it may not (e.g. electrons). In the last case, the existential assertion would simply be more deeply embedded in theory. Yet, in this case we still want to retain the *analogue* of the independent and direct verification principle which so straightforwardly applies to the former case. And clearly, scientific discourse expresses this most simply by an existential assertion (though its formalism will normally presuppose rather than express it). Only in this way shall we be able to distinguish the  $\phi$ -level refusal to admit the existence of  $x$  because of insufficient corroborative evidence, from the  $e$ -level refusal due to fascination with the inverse-deductive model of science.

The two approaches then exhibit radically different features. The  $\phi$ -level approach admits of differences of degree in the strength of conclusions based on scientific evidence. The  $e$ -level approach does not; the impossibility of discovering or even talking about 'the existence of', 'the reality of', the atom as such are based here on quite different considerations.

The extremely general nature of positions such as Mach's and Braithwaite's is not always appreciated, a consideration which will enable me now also to say something *for* them. Thus it might be urged that to think of an explanatory theoretical entity solely in terms of the deductive consequences to which the calculus in which it is employed leads, would make that entity, as it were, lack a life of its own. For (a) one gets a very weak sort of theory which one would be prepared to abandon as soon as it clashes with some fact or other; (b) one deprives oneself of making what have been called 'synthetic predictions';<sup>2</sup> roughly, those predictions which lead us to assign pro-

<sup>1</sup> I owe this remark to Mr W. Kneale

<sup>2</sup> See M. Born, *Experiment and Theory in Physics*, Cambridge, 1943, p. 10



## SOURCES OF SCEPTICISM IN ATOMIC THEORY

perties to the theoretical entity other than those given to it for the purpose of the original deductions.

These objections can be answered. Nothing prevents us from working with a theoretical entity considered as a mere concept or model; a model which may or may not bear a strong analogy to something else. All the Mach-Braithwaite view requires is that it should not make sense in such cases to *speak of existence* in a straightforward way! Indeed, this view may come back at the objector with a 'tu quoque'. For what, after all, determines the choice of properties of the theoretical entity on the *objector's* part? It is after all accepted on both sides that we are dealing with *theoretical* entities for the purposes of the argument; i.e. no straightforward sensory correlate exists!

For this reason some of Popper's objections, made to this sort of view (which he calls 'instrumentalist') seem to me somewhat queer. For he seems to think that this view can be refuted because it appears to contradict what is so obviously implied in all genuine scientific thinking, viz. that scientific theories can be 'true description(s) of the world';<sup>1</sup> that it is absurd to say that 'there can be nothing hidden';<sup>2</sup> that it is legitimate 'to explain the known by the unknown'.<sup>3</sup> Now it is true that some positivists have at times talked as though their views did entail the nonsense that there can be nothing hidden. But surely this cannot be the real issue. For *that* is not whether there can be anything hidden but what is to be the fundamental analysis of statements involving hidden, i.e. theoretical, elements. To show that *e*-level arguments clash with  $\phi$ -level *language* at most shows that there is something queer about *e*-level language; it does not *refute* the *e*-level argument which must be refuted on its own ground.

However, what is that ground? Views like those of Mach and Braithwaite have far more in common with a sort of quasi-grammatical legislation (*e*-level approach) than with the question whether something exists ( $\phi$ -level investigation). Mach is really saying: Thou shalt speak of existence in a straightforward sense only in cases of individual sensory elements; and the same goes *mutatis mutandis* for the other case. It is therefore interesting to find that Popper also brings forward what looks like a much more *general* reply to the Machian type of view, less concerned with what scientists think and more with the general structure of our experience. Let us remember the contention, he

<sup>1</sup> K. R. Popper, 'Three Views concerning Human Knowledge', in *Contemporary British Philosophy* (ed. H. D. Lewis), London, 1956, p. 358

<sup>2</sup> *Ibid.*, p. 368

<sup>3</sup> *Ibid.*, p. 364

says, that most scientific statements include more than reference to concrete particulars; they involve universals, disposition words, abstract words, and thus all the way down to nouns for such physical objects as 'this glass of water'. But, he goes on, these *all* have *descriptive function*. Since a language without universals could not work, their necessary use 'commits us to asserting, and thus (at least) to conjecturing, the reality of dispositions . . .'.<sup>1</sup>

Now it would not worry the instrumentalist to be told that even the most fundamental propositions of our language are pervaded by disposition words. And I think he would not mind admitting that by means of such propositions we often mean to 'describe' a certain state of affairs. But what he *would* deny is that from this it follows that disposition words had a 'descriptive function' in any *ultimate* sense, i.e. a sense that committed him to admit the 'reality of dispositions'! To use my terminology: whilst not denying descriptive function at the  $\phi$ -level he would not accept it at the  $e$ -level. Once again the two levels of discourse have failed to meet, through being confused. At most, what one has to admit is that Popper has called the Machian bluff, by *insisting* that since dispositionals have descriptive function at one level, they also have it at the other. To Mach's grammatical legislation he replies with an equally forceful piece of counter legislation. Nor could he be expected to have done more. It is this which marks the dispute here as belonging to the  $e$ -level. And to the extent to which it does, it is incurable by considerations belonging to the  $\phi$ -level.

As we see, this distinctness of levels is not always perceived too clearly. On the contrary, it happens quite often that an artful mixing of what is *logically* distinct has the effect of these two levels *causally* influencing each other. What then happens is that the 'logical strength' of one level is employed as a lever to secure a certain position at the other. It is now of great interest to the historian and philosopher of science to discover that this mixing of levels occurs at certain crucial periods of the development of a scientific concept itself. This confusion may hamper or accelerate the development; it almost always causes lively controversy and a rethinking about foundations.

## 2

I want to consider this matter in the historical context of chemical theoretical developments of the last century, in particular the sceptical

<sup>1</sup> Popper, *op. cit.*, p. 388

## SOURCES OF SCEPTICISM IN ATOMIC THEORY

conclusions on atomic theory reached by the famous French chemist J. B. Dumas and others, conclusions which were sufficient to shake the confidence of chemists in Avogadro's Hypothesis and the atomic theory in general.<sup>1</sup>

During the early nineteenth century the problem for chemists was the accurate determination of relative atomic weights, the difficulty being to get the ratios of the numbers in which atoms of elements combined to form compound molecules. To this end, by the mid-twenties the Swedish chemist J. J. Berzelius who had done the most important work here had developed methods involving such things as the employment of chemical analogies, multiple oxides, the formation of salts, the phenomenon of isomorphism, Dulong & Petit's law of atomic heats, and finally, one which he regarded as particularly powerful and which was based on the assumption that the number of atoms combining to form compounds, where these are formed from elementary gases, are proportional to the combining volumes of the latter. (He did not extend this number rule to the compounds themselves, a fateful limitation, as we shall see.)

Now Berzelius's approach towards the atomic theory was somewhat tentative. Thus, in his 1827 *Lehrbuch der Chemie*, the terms 'chemical equivalent' (a purely experimental concept) and 'atom' are sometimes used deliberately as synonyms.<sup>2</sup> Likewise, the expression 'volume' is to be used very nearly synonymously with that of 'atom', because the theories involved are

mere methods of representation for the combining elements, through which we better understand the phenomena, and one does not aim to explain thereby the processes as they actually take place in nature.<sup>3</sup>

Dumas, by contrast, at first enthusiastically and without reservations adopts the atomic theory, 'this admirable conception', whose results have become 'the basis of all chemical research that requires some accuracy'.<sup>4</sup> His aim is nothing less than 'to replace by positive notions the arbitrary ideas on which almost the whole of atomic theory rests'.<sup>5</sup> And then, ten years later, the dream had come to nothing.

<sup>1</sup> 'Avogadro's Hypothesis' says that equal volumes of all gases contain equal numbers of molecules under the same conditions of temperature and pressure.

<sup>2</sup> J. J. Berzelius, *Lehrbuch der Chemie*, III (i), Dresden, 1827, p. 31

<sup>3</sup> *Ibid.*, p. 44

<sup>4</sup> J. B. Dumas, 'Mémoire sur quelques points de la théorie atomistique', *Ann. chim. phys.*, 1826, 33, 337

<sup>5</sup> *Ibid.*, p. 340



'If I were master', he writes, 'I should erase the word *atom* from the science, convinced that it goes beyond experience; and never in Chemistry should we go further than experience'.<sup>1</sup>

The whole episode is too complicated to relate in any detail. The 'positive notion' which was to replace the 'arbitrary ideas' of the contemporary theorists was the extension of the same-volume-same-number-of-atoms rule to the case of all gases and vapours; in other words, something approximating (though not identical with) the hypothesis of Avogadro. An early difficulty was that it became necessary to 'represent water by one atom of hydrogen and a half-atom of oxygen'.<sup>2</sup> This seems to have shocked Berzelius who sarcastically writes:

Hitherto it has been usual to abandon an hypothesis as soon as it leads to something absurd; however in some cases this seems to be found too inconvenient.<sup>3</sup>

Why absurd? Simply because the distinction between molecule and atom had not yet become familiar, and because the notion of a half-atom *seemed* absurd, particularly when referring to elementary atoms. It should be noted that an hypothesis is here impugned for an apparent lack of plausibility—a notion hovering at the edges of the  $\phi$ -level.

Dumas's prime aim had been to determine the vapour densities of certain elements in order to calculate their atomic weights, using Avogadro's hypothesis. And this revealed further curious anomalies. The first he notes already in the paper of 1826 though apparently without further thought. The value of Mercury obtained by this method is only one-half of that obtained for the atomic weight by the methods of Berzelius and others. Later determinations of the atomic weights of Phosphorus and Sulphur yield further anomalies.<sup>4</sup> The value for Phosphorus comes out double, for Sulphur three times the accepted numbers.

Now these are the bare facts. What is interesting is that as the years go by they seem to have had a cumulative corrosive effect on Dumas's mind, gradually causing him to alter his views on the usefulness of the atomic concept. We should not of course imagine that he either *simply* accepted, or ignored, or rejected the difficulties mentioned. Moreover, his criticisms are mostly (though not altogether) directed

<sup>1</sup> J. B. Dumas, *Leçons sur la philosophie chimique*, Paris, 1878 (reprinted from the 1st edn. of 1837), p. 314

<sup>2</sup> Dumas, 'Mémoire', p. 339

<sup>3</sup> J. J. Berzelius, *Jahres-Bericht*, 7 Jhg., Tuebingen, 1828, p. 80

<sup>4</sup> Dumas, *Ann. chim. phys.*, 1832, 49, 210; 50, 170

## SOURCES OF SCEPTICISM IN ATOMIC THEORY

against the *chemical* (as distinct from physical) atom, and in connection with the employment of that concept for the purpose of weight-determinations. In his *Traité de Chimie* of 1828 he had more clearly expressed the idea that the 'atoms' of the elementary gases are themselves compound; and by 1832 this had led him even to suggest that the molecules of Phosphorus and Sulphur may be polyatomic,  $P_4$  and  $S_8$  in our language, rather than diatomic as he had hitherto assumed. This guess was not to be verified until thirty years later, by the studies of Deville and others on the phenomenon of dissociation.<sup>1</sup>

Unfortunately, at this stage of development of atomic theory the guess would seem somewhat arbitrary. The least Dumas had to do was to reconcile it with current 'established' general theory, in short: to make it plausible. Although 'arbitrary' additions of this sort often form the very nerve of scientific research, this is the price one has to pay. In the 1828 *Traité* he explains that we must 'regard it as demonstrated' that heat is less efficient than chemical action for dividing the physical molecules of the simple gases.<sup>2</sup> This was to be a fateful 'explanation' of his 'assumption'. For by 1836, to judge by the celebrated *Leçons sur la philosophie chimique*, it seems to have dawned upon him that here, too, there is a contradiction: for in the case of Mercury, chemical action seems *less* effective than physical! This, so he tells us, is really a 'shocking anomaly'.

Of course, he *need* not have reacted in this way to difficulties which the next generation was to brush aside as merely 'apparent exceptions' to Avogadro's law. But in 1836 Dumas could still, nay perhaps even had to, embrace the other alternative. To him, the whole theory no longer appeared 'useful', particularly since it involved the unverifiable assumption of a determinate number of atoms in molecules. This assumption, he writes, is 'after all nothing but an hypothesis, and on that subject one has already enough'.<sup>3</sup> And after further discussion, ranging over the remaining methods of weight determination, he returns to the theme: 'What remains of the ambitious excursion . . . into the region of the atoms? Nothing, at least nothing necessary (*sic!*). Indeed,

what remains is the conviction that chemical science goes astray as usual when it abandons experience and when it wants to walk without a guide through the shadows. . . . On hand of experiment you get

<sup>1</sup> Deville and Troost, *Ann. chim. phys.*, 1860, 58, 287 and 298

<sup>2</sup> Dumas, *Traité de chimie*, Vol. I, Paris, 1828, p. 39

<sup>3</sup> Dumas, *Leçons*, p. 291

the equivalents of Wenzel, of Mitscherlich, but you will in vain seek the atoms such as your imagination has dreamed up. . . . My conviction is that the equivalents of the chemists, such as Wenzel, Mitscherlich (i.e.) what we call *atoms*, are nothing else but molecular groups.<sup>1</sup> If I were master, I'd erase the word *atom* from the science, convinced that that goes beyond experience; and never in Chemistry must we go further than experience.<sup>2</sup>

There are several interesting features in Dumas's response. The most important of them is the forceful declamation against the use of hypothesis, the reminder that the atoms of chemistry are only hypothetical constructs, and that chemistry is ill-advised to use them—that they give us nothing 'necessary'. It is of course easy to read into this a straightforward philosophical complaint; but whilst (as I shall presently show) the echo of such a complaint can be heard in Dumas's reasonings, he is mostly carrying on his argument at the  $\phi$ -level. What for instance does he mean by 'going beyond experience'? The first and most important reason is the reminder that there is something dangerous in using the term *atom* to mean more than equivalent or volume, to use it as something more than a summary description of certain general facts. Now Dumas had gone beyond this and worked with a concept which he had endowed with certain attributes which for the time being had to appear as 'arbitrary assumptions', such as the postulate of diatomic molecules. As we have seen, subsequent investigation showed that this led to contradictions and could not be used.<sup>3</sup> A second difficulty had been to fit this assumption when surrounded by suitable provisos (polyatomic molecules) into his general thought-framework concerning the effectiveness of heat as against chemical action. A third consideration was that the assumption of diatomic molecules composing simple gases could only with difficulty

<sup>1</sup> By molecules, Dumas understands the *smallest physical* particles of which we may assume matter to be composed. They help us to understand isomorphism and certain heat phenomena; and they are the carriers of electric charge required for an 'explanation' of chemical combination in the theories of Davy, Ampère, and Berzelius. But Dumas has no positive molecular theory here either, and we may limit ourselves to a consideration of *chemical atoms*.

<sup>2</sup> *Ibid.*, p. 314

<sup>3</sup> As Dumas expressed it: it appeared to follow that 'gases, even when they are simple, do not contain, in equal volumes, the same number of atoms, or at least, of chemical atoms' (Dumas, *Leçons*, p. 291); which is of course a very worrying way of referring to the existence of different types of molecules. In the 1832 paper this had suggested to him that all approaches which had been using this hypothesis, including that of Berzelius, were misleading.



## SOURCES OF SCEPTICISM IN ATOMIC THEORY

be squared with Berzelius's electro-chemical theory: how could identical charges attract each other? (Notice that it is not so much that the facts collide with the assumptions; it is that assumptions have to run the gauntlet of existing theories, even though the latter may be more weakly founded than the former!)

Clearly, it was not the use of hypothesis as such that was reprehensible. So far, the main cause for complaint was simply that the properties that had been postulated had led to unverifiable predictions; secondly, that they did not cohere with the existing body of chemical and physical doctrine of the time. Dumas's mode of expressing himself therefore seems a little exaggerated—though it seems deeply to have affected his contemporaries. (On this effect, cf. E. v. Meyer's *History of Chemistry*.)<sup>1</sup> But I think that the mode of expression connects also with a subtle intermixture of epistemological and logical considerations which though they ought to be made a separate issue, are not clearly kept apart. Instead of being concerned with the insufficiency of the evidence, Dumas also seemed to have become affected by a consideration of its indirectness.<sup>2</sup>

The chapter of the *Leçons* from which I have been quoting is preceded by one devoted to a highly coloured and selective survey of the history of atomic theory. On the whole, the general impression which this chapter is meant to convey is one of acute scepticism. Interesting are the sort of objections raised. Thus, concerning Dalton, Dumas remarks that the latter only *assumes* the existence of atoms but does not prove it; he only shows that the facts are consistent with the assumption. Nor does the assumption require in the least that the atoms be actually indivisible smallest elements of matter. As to the second remark, it looks in two directions. One is technical: the assumptions of Dalton in fact do not preclude the atoms from having an internal structure. The other is of a more general kind. It harps on the logical difficulties which so many generations of philosophers have found with the concept.

The first remark is more clear-cut. It stresses the fact that the reasoning involved is hypothetico-deductive; it stresses the 'logical analysis' of the situation. Which is to give quite a different sort of reason why the atom involves mere hypothesis. It raises a complaint which can *not* be cured by any subsequent discoveries. It occurs in

<sup>1</sup> E. von Meyer, *A History of Chemistry*, 3rd Eng. edn., London, 1906, p. 237

<sup>2</sup> On this distinction, see G. Buchdahl, 'Science and Metaphysics', in *The Nature of Metaphysics* (ed. D. Pears), London, 1957, pp. 64-72

innumerable writers of that time. Typical here again is Berzelius. In the 1843 German edition of his *Lehrbuch*,<sup>1</sup> he tells us that he decided to use the atomic conception although it is an hypothesis for which we shall never have direct evidence even though all deductions from it check. And on the next page: The laws of combination must have a cause although this cause could 'never become the object of testing, in so far as it manifests itself through its effects'—the usual way in science; it cannot be proved or disproved directly.

It is nothing but an hypothesis and will probably always remain one; however, it follows elegantly from the facts; and on the other hand, when assumed to be fact, it supplies directions for important conclusions, like a theory that is completely proved.<sup>2</sup>

Here again, of course, it may be said that these remarks merely state the fact that for Berzelius the atom was hypothetical simply because it was not yet (or might never become) an object of direct inspection—leaving indeterminate the boundaries of the grammar of 'direct'.<sup>3</sup>

And of course one will be right: those complaints muddling two positions as they do, are not purely 'logical'. They are clearly associated with the idea of possible improvement in knowledge. Already Dumas gives evidence of this. He not only complains that Dalton has used inverse-deductive reasoning; he objects to Dalton and Swedenborg that their atomic theories merely cover the known facts and predict no new ones. Within the restricted field of the chemical knowledge of the thirties this complaint was bound to remain endemic. Here, the discoveries of the subsequent thirty years in organic chemistry and (even more important) those in the physical theory of gases leading to an independent deduction of Avogadro's law were bound to change the entire aspect of the matter. Indeed, it was developments in this field of kinetic theory which for many scientists converted the 'atomic hypothesis' into something more definite; or speaking more formally: which converted the language of hypothesis into the language of fact, notwithstanding its failure to satisfy any better Berzelius's demand for 'direct evidence'! Merz, writing in 1896 (influenced by ideas first formulated about thirty years earlier) expresses it very clearly:

Originally suggested only to explain, describe, or symbolise the fact that different substances combine in fixed, and especially in fixed

<sup>1</sup> Berzelius, *Lehrbuch der Chemie*, 5th edn., Dresden, 1843, Vol. I, p. 10

<sup>2</sup> Berzelius, *Lehrbuch*, p. 12

<sup>3</sup> See Appendix

## SOURCES OF SCEPTICISM IN ATOMIC THEORY

multiple proportions . . . [it was the kinetic theory that forced philosophers to take] seriously the opinion that molecules and atoms existed in reality, and were not merely convenient symbolisms, as many great chemists during the first half of the century were inclined to think.<sup>1</sup>

Here 'dynamical reasoning' is made the touchstone on which turns the difference between the atom's merely 'symbolic' existence as against its possible possession of a more substantial reality—obviously a  $\phi$ -level approach. Merz here echoes Maxwell who had emphasised that whilst the chemist's reference to equivalents in terms of atoms may possess a certain 'cogency' it is but 'purely chemical reasoning; it is not dynamical reasoning. It is founded on chemical experience, not on the laws of motion'.<sup>2</sup>

Technically speaking, therefore, coming to the atom from the side of dynamics constitutes an immense improvement, even a 'proof' of its reality. Putting it formally: these writers exhibit the accepted use at the  $\phi$ -level, of the expression 'the atom exists'. However, and for this reason, we shall not be surprised that the logician and epistemologist is not to be won over so lightly. At the  $e$ -level he uses quite different tools!

Perhaps one of the clearest indications of this position is to be found in Lange's *History of Materialism*, written in the wake of these momentous developments.<sup>3</sup>

We may maintain that 'Atomism' is proved, if by this we understand nothing more than that our scientific explanation of nature in fact presupposes discrete particles which move in at least comparatively empty space. . . . On the principles of the hypothetical deductive method, we can say with Clausius and Maxwell, *if* matter consists of discrete particles, they *must* possess such and such properties. . . . The theory which rightly explains, and even predicts, the facts, may, indeed, thus gain so much probability, that for our subjective conviction it comes very near to certainty; but still always only supposing that there can be no other theory which will do the same.<sup>4</sup>

Here we see very clearly that the *logician's* language is not necessarily modified by obvious inferences made at the  $\phi$ -level, concerning

<sup>1</sup> J. T. Merz, *History of European Thought in the Nineteenth Century*, Edinburgh, 1896, Vol. I, p. 432

<sup>2</sup> J. C. Maxwell, 'Atom', *Enc. Brit.*, Vol. III, 9th edn., Edinburgh, 1875, p. 40

<sup>3</sup> The first edition of this work appeared in 1857, the second in 1873-75, owing a great deal as one can see from the footnotes to Lothar Meyer's atomistic work *Modern Theories of Chemistry*, 1st edn., 1862, 2nd edn., 1872.

<sup>4</sup> F. A. Lange, *The History of Materialism*, London, 1880, Vol. II, pp. 383-384



the reality of scientific entities. At the logical level, we find that the indirectness of the atoms is so radical that nothing that is done at the  $\phi$ -level will ever produce them.

This evaluation of Lange's logical position is corroborated by a study of his epistemological views which are Neo-Kantian and which seem to saddle the atomists with the doctrine that atoms are a species of 'things-in-themselves', from which it is of course not difficult to deduce that atoms are not the sort of things to which the language of physical existence could in principle apply. Once again we realise the generality of the argument which despite appearances to the contrary, has moved on to the  $\epsilon$ -level.

## 3

As this case history illustrates,  $\phi$ -level discourse covers a whole spectrum of distinctions stretching from treating a theoretical entity as a mere symbol, to a whole-hearted acceptance of its physical reality at the other extreme—all of them based on complicated factual and theoretical considerations. And, as I have suggested, the epistemological argument, by comparison, yields a relatively blunt conclusion, coming out, for instance, on the side of phenomenalism as against its opposite, critical realism, or *vice versa*. And as such it may exert *some* causal influence upon the historical development of the relevant concepts, despite the fact that *logically* the epistemological position may cut right across the conclusions of the scientific enquirer. The success or failure to demonstrate scientifically the existence of a theoretical entity like the nineteenth-century atom therefore has very little if any *logical* bearing on the reasoning of the epistemologist; nor can the latter avail himself of scientific successes in this field to bolster up his arguments on behalf of his own philosophical theory. To illustrate the former case: The epistemologist's conclusions (based on *logical* arguments) that *every* scientific proposition presupposes elements reaching beyond immediacy of sensation, has no *logical* relevance to Dumas's doubt concerning the 'merely hypothetical nature' of atoms—in so far as these doubts were based on scientific considerations, and not (as indeed in part they were) on a philosophical view. Finally, although the *logical* independence of the two sorts of arguments mentioned must by now be evident, it will be equally clear that they have exerted considerable mutual influence on each other—whether for better or worse it is difficult to say.

## APPENDIX

*Berzelius and Hypothesis*

The fear to go beyond 'experience' and use hypothesis had fantastic consequences for the terminology and ideological background of the chemists of the thirties and forties. Thus many chemists, e.g. Berzelius, use the terms atom and volume, or again, atom and equivalent interchangeably, partly for reasons stated in the paper.<sup>1</sup> Consider in this context Gmelin, author of the famous *Handbuch der Chemie*. Having in the earlier editions been unable to accept the atomic theory, in the 1843, 4th edition, he writes that he has 'decided to adopt the atomistic hypothesis, and hence (has) exchanged the equivalent weights for the atoms'.<sup>2</sup> But it is only an hypothesis, and so is the determination of the number of atoms in the molecules, both of simple as well as compound substances.<sup>3</sup> However, because of the difficulties and anomalies discovered by Dumas,<sup>4</sup> and because one now has to distinguish between molecules and atoms for the case of elementary gases<sup>5</sup> Gmelin does not accept this. He announces proudly, that in his system 'equivalent and atom are identical and all confusion is avoided'.<sup>6</sup>

In this of course he deceived himself. For one could hardly ignore the evidence from Gay-Lussac's combining volume figures. Here, for instance, two volumes of hydrogen combined with one volume of oxygen. If there was anything in the atomic hypothesis, then if you assumed a formula HO for water-vapour (as Gmelin, following Dalton and many members of the English school, was doing), you must assume that double the volume of hydrogen contains the same number of atoms as a volume of oxygen. To do this, says Berzelius, Gmelin and his friends simply assume that in such combinations 'one volume of oxygen-gas encloses twice as many atoms as one volume of hydrogen gas'.<sup>7</sup> But—

It is obvious that this explanation cannot be demonstrated by direct experiment; it is consequently nothing but a simple supposition imagined to prop up a preconceived theory. Although one seeks to base

<sup>1</sup> See pp. 125-6

<sup>2</sup> L. Gmelin, *Handbuch der Chemie*, Heidelberg, 1843, Vol. I, p. 4

<sup>3</sup> Ibid., pp. 45, 46

<sup>4</sup> Ibid., p. 47

<sup>5</sup> Gmelin puts this by complaining that one would be forced to the assumption of 'such small atoms as are never encountered in any combination' (Gmelin, op cit., p. 47).

<sup>6</sup> Ibid., p. 48

<sup>7</sup> Berzelius, *Traité de chimie*, 2nd Fr. edn., Vol. IV, Paris, 1847, p. 502

theories on facts, in order to avoid hypotheses, one nevertheless can make mistakes, because one does not know all the different facts which must contribute towards a complete demonstration. Nonetheless, the mark of a fact which necessitates the establishment of an hypothesis presents always a greater certainty than a simple supposition.

I have quoted this passage in full, in order to indicate something about the subtle variants on the notion of hypothesis, proving an hypothesis, making a supposition, etc., current in the literature. At any rate, the distinction here drawn between facts that 'necessitate hypotheses' and 'mere suppositions' is noteworthy for the thesis of this paper. Moreover, even so, the matter is not as clear-cut as Berzelius makes it seem. For on the one hand, he does not mind occasionally making some arbitrary assumptions himself in his criticism of Dumas concerning the volume relationships between gases; and on the other hand, Gmelin had of course thought that *his* 'arbitrary supposition' *at least* was compatible with the theoretical conceptions concerning the nature of heat then held by himself and many of his friends.<sup>1</sup> Where Berzelius had refused to extend the Avogadro rule to compound gases, Gmelin even refused to apply it to elementary gases; arbitrarily suggesting that the requisite number of atoms was a function of their diameter considered as due to the atom plus its surrounding heat-atmosphere. And if this seems arbitrary, Berzelius had not minded 'arbitrarily' making the force of repulsion between the compound atoms come out to give the required volume relationship coincident on the formation of compounds from gases, so as to have an alternative to the consequences of Avogadro's rule and Dumas's use of it.<sup>2</sup> My main point here is that this language of facts relating to theory, supported by either theory or arbitrary assumptions, and so on, entirely moves within a realm where no doubt is cast upon the fundamental logical distinctions involved. And this must be rigidly distinguished from the other approach, which makes Berzelius introduce the *whole* of his theoretical discussion of which the arguments just mentioned form a part, with the words:

Every theory is only a manner of representing the nature of the phenomena.<sup>3</sup>

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<sup>1</sup> Gmelin, *op. cit.*, p. 47

<sup>2</sup> Berzelius, *Lehrbuch* (III) I, 1827, pp. 47-8

<sup>3</sup> *Ibid.*, p. 28; also in *Traité*, 1847, p. 484



## EPISTEMOLOGY AS AN AID TO SCIENCE : COMMENTS ON DR BUCHDAHL'S PAPER\*

J. AGASSI

. . . that duty to science, which  
consists in the enunciation of  
problems to be solved. . . .

FARADAY

I FOUND some difficulty in appraising Dr Buchdahl's paper. My difficulty is perhaps connected with the fact that I failed to recognise any similarity between his presentation of Popper's views and these views as I understand them. And I was at a loss to see why he should claim that Popper and Braithwaite are philosophically opposed to each other. For all I know their philosophical views agree very well on almost any issue which may be relevant to the present discussion.<sup>1</sup>

The main point of Dr Buchdahl's paper has become somewhat clearer to me after long discussions with its author and Mr Gellner. I have ultimately assumed that Dr Buchdahl's thesis is that epistemological discussion is entirely useless for scientific research, a thesis very much in tune with the philosophy now in vogue in Britain. I shall try to criticise this thesis of Dr Buchdahl's, and to argue that in the historical case which he chose to discuss as an illustration of his point—Dumas's views of atomism—epistemological criticism might have

\* This paper was read on Sunday, September 21st, 1958, to the third Annual Conference of the Philosophy of Science Group at Oxford. Apart from stylistic alterations and some cuts, only the introduction was altered, and footnotes were added. I am grateful to Mr L. P. Foldes who read the MS. and made many helpful suggestions, and to Mr Ernest Gellner for helpful elucidation.

<sup>1</sup> The following quotation, from the penultimate paragraph of R. B. Braithwaite's *Scientific Explanation* (Cambridge, 1953) may illustrate this agreement: 'If we interpret the calculus as being a scientific deductive system, we hand over to Nature the task of deciding whether any of the contingent lowest-level conclusions are false. This objective test of falsity it is which makes the deductive system, in whose construction we have very great freedom, a deductive system of scientific hypotheses. Man proposes a system of hypotheses: Nature disposes of its truth or falsity. Man invents a scientific system, and then discovers whether or not it accords with observed facts.'

been of great use as a tool for solving scientific problems. The point of view from which my criticism is launched is that of Popper, as I shall explain in the body of my discussion.

Indeed, Braithwaite in the last and most prominent sentence of his *Scientific Explanation* anticipates my reply to Dr Buchdahl's thesis. For there he expresses his views on the possible influence of his epistemology on scientific research as follows:

If this clarification has the secondary effect of encouraging scientists to construct deductive systems and to use theoretical concepts freely, it will, I believe, assist the progress of science as well as the better understanding of what science is doing.

## I

If I understand Dr Buchdahl rightly, his point is this. Discussions of scientific problems cannot gain but can only lose from epistemological arguments. An epistemological argument is one which applies equally to all hypotheses as such, while a genuinely scientific argument concerns a given hypothesis or a given group of hypotheses. Of course, nothing prevents anyone from being interested in both epistemology and science. Yet one should not, he holds, use arguments from one field in the other. The pointlessness of doing this may be well exemplified by the case of a scientific discussion concerning the choice between two specified hypotheses. Any epistemological criticism of one of them—being general in nature—may be equally applicable or equally inapplicable to both, and is thus useless for the purpose of that discussion and even confuses the issue. Therefore we should accept as a general rule not to mix science with epistemology. I shall take this maxim to be what Dr Buchdahl recommended, although he himself seems to violate it—a fact which I shall ignore.

Dr Buchdahl will readily admit that some scientists who fell prey to this confusion were occasionally stimulated by it. Yet this is a mere psychological fact or a historical curiosity. The logic of his case is that an argument quite clearly belongs either to science, or to epistemology, or to neither, but not to both. Science *qua* science is not concerned with epistemological generalities.

This is my summary of Dr Buchdahl's position. It is surely a very important position, if only as a variant of the ideology behind the increasing specialisation of the present day. This position—epistemology for epistemology's sake—seems to me to imply that epistemology

is barren. (Some might feel that had they accepted Dr Buchdahl's view they would have dropped their interest in epistemology.) Yet I see no reason to accept Dr Buchdahl's grim judgment. First of all, it is very difficult to judge epistemology as such since we do not know its future contributions, much less their impact on scientific research. Moreover, the history of scientific research is full of examples of developments which were made possible through epistemological discoveries. I would mention the examples of Galileo, Boyle, Faraday, and Einstein. However, my present aim is to explain the inadequacy of Dr Buchdahl's suggestion that scientists should go on doing research without paying attention to epistemological arguments.

Any activity, be it scientific research or not, is based on the choice of an aim, an appraisal of the situation, and a decision as to the behaviour most likely to forward that aim in the given situation. The aim, the appraisal of the situation, and the decision are all necessary if the behaviour is to constitute rational activity. A critical attitude towards an activity implies readiness to accept criticism concerning any part of it. The criticism may be moral, i.e. concerning the desirability of the aim, or factual, i.e. concerning the correctness of the appraisal of the situation, and the adequacy of the decision. Now, when the activity in question happens to be scientific research, or the advancement of learning, the criticisms, concerning aims, circumstances and method, belong partly to ethics and mainly to what is called epistemology, using that word either in its traditional meaning, or in the meaning given to it by Dr Buchdahl. Therefore Dr Buchdahl's claim that epistemology cannot affect scientific research would render scientific behaviour uncritical and merely habitual. It would also deprive its practitioners of their personal responsibility. However, since we usually suspect that scientific research *is* rational, we may well suspect that Dr Buchdahl's judgment is false, since it implies that epistemological criticism is barren.

It is quite true that for the purpose of choosing a given hypothesis, and for other purposes, some epistemological considerations may be irrelevant. Yet the very idea that research involves a choice of a hypothesis is in itself an epistemological idea. Dr Buchdahl tells the investigator to go on doing scientific research and ignore epistemology, yet in research he includes the choice of hypotheses; which is a specific epistemological theory. Consider an investigator who has a different epistemological theory; say, one who believes that research does not involve any choice of hypotheses or any choice at all (since



choice contains an element of arbitrariness), but rather verification, or proof, or the compulsion of scientific statements. It would be foolish to tell this person to go on trying to verify his hypotheses. It would be much more reasonable to advise him to stop after a while and ask himself why all his attempts to verify his hypotheses have so far ended in failure, and whether these failures are not inevitable.

Dr Buchdahl would have told a verificationist like Dumas, who worried about his inability to verify any of his hypotheses, not to consider such epistemological problems, but rather to go on doing research. Yet by research Dumas understood trying to verify hypotheses. Therefore the only way to put him right would have been to advance epistemological criticisms, namely to explain to him the impossibility of verification. Yet according to Dr Buchdahl's position, such a criticism would have been redundant, since, according to him, everybody understands perfectly well the meaning of scientific research.

In this respect Dr Buchdahl's view is very similar to that of Wittgenstein, as Mr Gellner has pointed out to me. Wittgenstein asserted in his *Tractatus* that his own epistemological theory—good old verificationism—was self-evident and unassailable ('it *shows* itself') in a manner which makes it both impossible and unnecessary to state it. Now, the standard criticism of Wittgenstein's view, first advanced by Russell, is this: Wittgenstein's epistemology entails its own meaninglessness; yet he managed to state it, and even to claim for it the status of unassailable truth; which is absurd. Dr Buchdahl's epistemology can be viewed as watered down Wittgensteinianism. For him, epistemology is not inferior to science with respect to meaning; it is equally meaningful, yet equal-but-separate. Instead of Wittgenstein's thesis that epistemology is meaningless, Dr Buchdahl advocates the thesis that epistemology is pointless at least as far as science is concerned. Yet the same criticism which can be levelled against Wittgenstein's doctrine of meaninglessness, namely that since it renders itself meaningless it is absurd, can be applied to Dr Buchdahl's doctrine of pointlessness, namely that since it renders itself pointless it is absurd. For if all epistemology is pointless, so is Dr Buchdahl's epistemology. Therefore, if we accept Dr Buchdahl's suggestion that we go on doing research without paying heed to epistemology, we should have to ignore his own epistemology too, and go on doing research in the way it was done by most thinkers from Galileo to Niels Bohr, namely, by mixing science with epistemology. Thus Dr Buchdahl's view is inconsistent.

## EPISTEMOLOGY AS AN AID TO SCIENCE

On this point Dr Buchdahl's view resembles that of Polanyi, as Mr Watkins has pointed out to me. Polanyi thinks that research is not based on an explicit epistemological theory; indeed, not even on an expressible one. The epistemology implicit in research is that which can be transmitted only by personal contact between master and apprentice. Therefore there is no room for epistemological theory. Perhaps I can follow Polanyi's version more easily than that of Wittgenstein because in condemning the existing epistemological theories as such Polanyi rules out the possibility of any alternative to them, while Wittgenstein condemns the existing epistemological theories as such while advocating one of them—verificationism. Moreover I accept Polanyi's view that research is a traditional activity. I also follow him in holding that the scientific tradition is not completely describable in human language, simply because no fact—not even the apparently simplest fact—is completely describable in human language. Yet just as we try to describe facts about cabbages and kings, we may try to describe facts about the scientific tradition. Popper attempts to describe it as the tradition of criticism—of clashes of opinions. The criticism in question may relate to scientific or to epistemological statements. The question of when a particular criticism is scientific and when it is epistemological is an interesting and important question which Popper ventured to answer, yet it is not the major question. The main aspect of Popper's viewpoint, at least to me, is his advocacy of the critical attitude as such. Criticism is always welcome, whether it is labelled 'scientific' or not.

Polanyi's position—which is to me very stimulating though uncritical, or as he calls it 'post-critical'—rests on the following point. What Kant calls tact, what he calls personal knowledge, and what may be called a sense of discrimination, is something one learns by joining the tradition—something learned not as one learns the Latin word for 'tiger', but as sculpture and painting are learned. This point seems to me to be true and very important. Yet this personal knowledge which we learn in the scientific tradition concerns, among other things, the choice of what should be criticised next and the ability to criticise it. If Polanyi held his view of personal knowledge consistently, he would have said that it is a matter of personal knowledge to judge when to blend science and epistemology, and in what proportion. Furthermore, what I particularly do not accept is Polanyi's idea that one needs personal knowledge in order to judge the fruits of other people's personal knowledge. True, some sense of discrimination is

always necessary; yet we often need not have very much sense of discrimination in order to be able to appreciate critically the fruits of the labour of a person with the highest degree of ability.

This is the reason, it seems to me, why it is so easy to be wise after the event. If we do not wish to be wise after the event we should not blame Dumas (whose ideas Dr Buchdahl discussed) because he did not criticise his own verificationism, though we may appreciate all the more the important contributions of his friend Faraday which were made possible by such criticism. Faraday had more sense of discrimination and more critical ability than Dumas, but this only became obvious after the success of his criticism. Faraday's superior ability should not be used against Dumas, especially since Dumas also was critically minded.

I say all this in order to stress that I do *not* think that in general epistemological criticism is more important or less important than scientific criticism. If I say that I would rather have criticised Dumas's verification than his atomism, I say so for two very specific reasons. First, Dumas himself succeeded in criticising his atomism, but failed to criticise his verificationism. Secondly, it was this failure, this insistence on verificationism, which got Dumas bogged down; he was intimidated by it; he considered it disreputable to deal with unverifiable hypotheses. Indeed, his friend Faraday also got bogged down at the same time, and for similar reasons. Only after ten years of disappointment did Faraday start to develop his criticism of verificationism and thus got new wings. But until Popper, epistemologists continued to preach verificationism to the scientist. When I say that Dumas might have advanced his faculty for research by criticising his own epistemological views and thus freeing his imagination, I think this merely because I have benefited—I hope—from the fruits of later criticism, by Faraday and by the followers of his tradition—in which I include Einstein and Popper. I think that Dr Buchdahl has also somehow benefited from this tradition, and he has used its fruits in his quasi-definition of the 'scientific level'. (He does not define it as the domain of certainty!) However, he takes the fruits of later criticism of verificationism so much for granted that he regards those pronouncements of Dumas which he quotes, as irrelevant to research and as mere confusions.

Quite unintentionally, I am sure, he exhibits that attitude of superiority which some epistemologists show towards some serious scientific work, thereby causing an unhealthy hatred of epistemology.



Dumas was not confused, though like all humans he committed some errors, including his belief in verification. If he mixed science with epistemology—not enough for my taste, and too much for Dr Buchdahl's—all we can do as historians is to explain his behaviour critically and respectfully or leave it alone.

## 2

Before trying to explain Dumas's verificationism on alternative lines, I wish to add, somewhat parenthetically, a few words on Dr Buchdahl's criticism of Popper.

Dr Buchdahl claims that he accepts Popper's view about the aim of science being the search for the hidden reality only after having demoted it to the 'scientific level'. On the 'epistemological level' he doubts it. This puzzled me greatly. For both Popper and Mach have theories about the aim of science and the content of hypotheses. This is why I naïvely considered both Popper's and Mach's theories to be epistemological. Now Dr Buchdahl puts Mach's theory on the epistemological level and Popper's theory on the scientific level. This, Mr Watkins has remarked, would imply that only Mach's epistemology must be barren but not Popper's, which being on the scientific level can have an impact on research. To this I must say that double injustice is done to Mach. First, Dr Buchdahl overlooks the fact that Mach's epistemology also could have—nay, did have—a profound impact on research. Secondly, Mach and Popper are opponents and therefore Mach should have equal chance of being considered and perhaps accepted. Mach says that scientific theories do not mean what they seem to mean but rather that they are highly condensed reports of observations, while Popper says that they do indeed mean what they seem to mean, for they are conjectural assertions about the (unobserved) structure of the world. It is therefore unfair to Mach to tell students of scientific problems that they should ignore Mach and take Popper for granted. Yet Dr Buchdahl puts Mach's theory on the epistemological level, Popper's on the scientific level, and tells people to ignore the epistemological level. This, obviously, amounts to telling them to ignore Mach's theory and to accept Popper's alternative uncritically. I believe it is more rational and more just to argue about the alternatives critically than to reconcile them by delivering one to the scientist and the other to the epistemologist.

For lack of time I shall not consider here the reasons why verificationism was accepted in the first place. I shall only discuss the reason for its persistence although it was refuted again and again.

The way to dodge refutation, which Popper calls the conventionalist stratagem, was beautifully exemplified by Polanyi, who said something like this. A theory is scientific—I should say critical or rational—if when it is refuted it is considered to be false. Once the person who applies the theory (not the theory itself) is blamed for the false prediction which he deduced from the theory, that theory ceases to be scientific. This is a nice example of the conventionalist stratagem—of the method of having a scapegoat ready in order to save the theory. The Baconian conventionalist stratagem which I shall now discuss is a particularly clever one; for it is itself an attack on the conventionalist stratagem, as I shall now explain.

It was Telesio who was the first among the moderns to claim that the senses cannot mislead us. His friend Patricio criticised him by claiming that we do not see facts as they are but rather as interpreted by our theories. This criticism was fully accepted by Galileo, who subsequently rejected the authority of the senses, and by Bacon, who did not. Applying the conventionalist stratagem, he said that when someone sees facts not as they are but as interpreted by his theories, then it is not the fault of his senses but his own fault. It is his own *fault*, because he does so in order to save his theories from refutation. Observational errors always stem from observations containing some theoretical ingredients. Such observations are bound to mislead us, for the rôle which theory plays in interpreting observation is that of distorting sensations in order to fit them into the preconceived theoretical scheme so as to save that scheme. The observer must so act because he is simply unable to admit error. He is unable to see facts as they are, being bound to look for confirmations, in order to try to flatter himself that his hypothesis was correct. He will thus become prejudiced or superstitious, since he will not be able to see the evidence against his theory.

Today when so many of us so readily admit error it is a little difficult to understand Bacon's view and its impact. But it might be helpful to mention two examples of the behaviour which Bacon described and which were very widespread in his day. The examples are dogmatism and neuroses. We know that dogmatic people cannot but see

## EPISTEMOLOGY AS AN AID TO SCIENCE

facts as wonderful confirmations of their views. We know that neurotic people, when their neuroses are challenged, may distort the facts in a most fantastic way, rather than get rid of their neuroses or reject their own rationalisations of their neuroses. According to Bacon, all hypotheses sooner or later turn into neurotic obsessions and into dogmas.

The reason why Bacon's doctrine of prejudice was so popular, in spite of its great disregard of man's critical faculty, is that in many ways it was amply confirmed by experience. Since error was identified with prejudice people could either deny that a theory was erroneous, or declare it to be a prejudice. When Lavoisier's theory was successfully criticised by Davy, great natural philosophers like Gay-Lussac refused to declare that it was an error because they knew that Lavoisier was not prejudiced. Thus, they stuck to their error and became prejudiced, as Bacon predicted, just because they accepted Bacon's theory. (This may be an instance of what Popper calls the Oedipus effect.) People who accepted Davy's criticism of Lavoisier declared Lavoisier's theory to be a prejudice. Even such appreciative people as Dr Thomas Thomson and the young Faraday declared Lavoisier's theory to be a prejudice. Such declarations were, of course, based on Bacon's doctrine; but they were soon taken as evidence for it. Later many historians who rightly praised Lavoisier's theoretical contributions made no allusion to the fact that he had also made some mistakes. Accepting Bacon's doctrine, they thought it would be derogative to mention Lavoisier's errors. Thus, through the acceptance of Bacon's doctrine the picture of the great heroes of science became Baconian: they were painted as unerring supermen—as indeed they would have to be if Bacon's theory was true.

Another cause of the confirmation of Bacon's doctrine is this. A theory—say, Lavoisier's—is refuted by a new fact—like the existence of combustions without oxygen. After the refutation of the theory the refuting fact becomes plainly obvious and even commonplace—like the combustion of metals in sulphur, or of hydrogen in chlorine. How is it then that the great experimenters of the past—such as Lavoisier—failed to see such simple facts? Answer: because they were blinded by their own theories. Ergo, they were prejudiced.

Incidentally, all this is a very good example which itself confirms Bacon's view; for the sheer acceptance of it made people see the history of science in its light. Yet this example also shows that Bacon's theory did not solve the problem of dogmatism. Bacon himself still



believed that some kind of confirmation is possible, instead of seeing, as Galileo<sup>1</sup> did, that what matters is not confirmation but criticism. Bacon's antidogmatic theory was soon confirmed and became *the* dogma and therefore was hardly ever criticised. It ceased to be the 'idol of the market place' only after Einstein's theories were taken seriously. It was later restated in various forms, say, by Mannheim and by Warnock who recently attributed it to Wittgenstein.

I would briefly mention Popper's criticism of Bacon's doctrine. First of all, none of us can be free of prejudice. Science is made by people, who, being human, have their own prejudices. These need not be impediments, as Bacon thought; they can be useful. Even if one person is prejudiced by a theory, says Popper, that theory may be a challenge for another to try to criticise it, and perhaps to discover the refuting instance. The refuting instance would not have been discovered at all but for the existence of that theory, and of people who are prejudiced by it, and the subsequent attempt to criticise it. Perhaps the prejudiced person will stick to his view in spite of refutations, but these may be taken more seriously by other people. Perhaps, as Planck said, only the next generation might reconsider the situation and take the criticisms more seriously; yet in one way or another the criticisms may be taken seriously and thus bring progress.

Although I reject Bacon's doctrine of prejudice my contention is that it is a very plausible doctrine, which the traditional natural philosophers took very seriously. These facts, I think, help to explain their attitude. Their acceptance of Bacon's doctrine explains their effort not to go beyond sense experience—their effort not to be committed to false views. This is why I think that I could have argued with Dumas on the lines which Dr Buchdahl rejects.<sup>2</sup> The point I

<sup>1</sup> As Dr Crombie remarked in the discussion, Galileo too looked for experimental evidence deciding for his Copernicanism. Yet what he looked for was a clear cut *crucial* experiment, and he surely thought that criticism is more important than supporting evidence, much as he would have liked to have both.

<sup>2</sup> This is a reference to Dr Buchdahl's claim that 'it would be somewhat of a joke' to tell Dumas that verificationism is false. See his concluding section. Since Dr Buchdahl's present conclusion is somewhat different from the one he read at the conference, I should like to add that his present position is more manifestly irrational. He still insists that epistemological criticism is irrelevant to research, yet he now (though not in his original version) agrees that this kind of criticism may still be influential. No doubt, criticism, as any other activity, may be influential. Yet we like to believe that criticism influences us in a different way than cajoling or threats. Once Dr Buchdahl denies that a certain kind of criticism may be effective in the way

should have made to Dumas is that he could not escape from error, and that error need not be so dangerous as it seemed to him; that his atomism, though erroneous, was not disastrous (as he thought it was, as it is clear from Dr Buchdahl's quotations) since it was criticised and refuted. I believe that realising this he might have been less worried, and less paralysed by the fear of error. This is just one example of the possibility that epistemological criticism may have a liberating effect. Indeed, although Faraday remained to the last a Baconian to some degree, he was liberated by his discovery that even the most cautious man is not exempt from error, and that research cannot progress without error. He was the first to rebel against the Baconian tradition by publishing systematically his own errors even after he had refuted them experimentally. He warned people not to accept his speculations but to use them critically, to try to test them experimentally. All he claimed was that his own views, limited though they might be, were not refuted while those of the majority were. Faraday's free style of writing was such a novelty that years after his death Maxwell commented on it with a great sense of surprise, admitting its helpfulness to the reader, and yet proposing to follow Ampère's Baconian style, rather than Faraday's. Fortunately, Maxwell himself did not follow his own proposal and a whole school of free expression of thought developed, culminating, I believe, with Einstein. In 1905 Einstein published two theories, one of which asserts that light consists of continuous waves, the other that light consists of discrete quantities of energy. Einstein knew very well that for mere logical reasons at least one of these theories was in error. Yet he published both, leaving it to criticism to sort out the true from the false.

It is easy now, after Einstein's new tradition and after Popper's rationalisation of it, to look back at Dumas and to claim that his worries were due to a confusion of science with epistemology. By the same token one may pooh-pooh even the famous 'crisis in physics' as mere epistemology which has nothing to do with science as we

other criticisms may be effective he implies that the influence ('causal influence' he calls it) is not necessarily connected with the validity or invalidity of the criticism: on the contrary, he denies that the influence may be 'logical' or based on the validity of the criticism. Ergo, although Faraday's criticism of verificationism was valid, Faraday's taking account of it in his research was, according to Dr Buchdahl, merely 'causal' and not 'logical'. I am quite certain that Dr Buchdahl will either deny that this is a valid conclusion from his concluding section or else admit that his concluding section is mistaken.

know it now (as Dr Buchdahl does by implication). Yet this 'crisis' was the great intellectual event of liberating us from verificationism—the new Renaissance—which opened the way for our new epistemology. True, no epistemology would have solved all problems unless it is a mock-epistemology. Science is trial and error, and in order to progress one has to try. The new epistemology would not have solved Dumas's problems; yet it would have helped him. Admittedly, freedom of thought and readiness to accept criticism are always characteristics of the scientific tradition. Yet Faraday's view that science should be in a permanent crisis is still a novelty. And Popper's idea that if one is successfully criticised one need not be discouraged but should be grateful and try again—this idea, too, I am afraid, is still far from being very popular. Not only could it have helped Dumas—it can still be of service to those who have not yet considered it. In this respect the scientific tradition (which I greatly admire) seems to me to be in need of reform.

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## REVIEWS

*Perceiving: A Philosophical Study.* By Roderick M. Chisholm.

Cornell University Press, Ithaca, New York, and Oxford University Press, London, 1957. Pp. xii + 203. 22s.

WHEN the reader opens this book he may be a little surprised to find that more than a third of it is concerned not with perception but with belief. Part I is called 'The Ethics of Belief'. The discussion of this subject is continued in Part II, chapter 7, while the last chapter of all, chapter 11, 'Intentional Inexistence'<sup>1</sup> is again concerned with belief, but this time with the epistemology and psychology of belief rather than its ethics. Why should there be such an elaborate discussion of belief in a book about perceiving? Professor Chisholm's answer is that all empirical evidence (all empirical grounds for belief) comes from perception and memories of past perceptions. So a discussion of perception is the inevitable sequel to a discussion of the 'ethics' of belief, and a discussion of the 'ethics' of belief is the inevitable preface to a discussion of perception. (But what of the 'ethics' of our beliefs about ourselves? Surely we sometimes have good evidence for these as well? I would suggest that Professor Chisholm should now write a companion volume on Introspecting or Self-consciousness.)

Professor Chisholm has many interesting things to say both about belief and about perception, and some of them are highly controversial. There is much originality not only in his detailed treatment of these two well-worn subjects, but also in his general manner of approach to them. This is a book which really does deserve that rather overworked epithet 'thought-provoking'. Unfortunately it is difficult to summarise Professor Chisholm's views without distorting them. He writes very well, but also very carefully. The analyses he offers are sometimes complicated, and the reasoning by which he supports them is often intricate. I can only attempt to give a broad outline of his main argument, omitting many interesting details and ignoring many refinements.

I shall first discuss his views on the Ethics of Belief (The phrase 'Ethics of Belief' was first used, I think, by W. K. Clifford<sup>2</sup>). Professor Chisholm

<sup>1</sup> This is a technical term of Brentano's. The question is roughly whether beliefs have 'objects'. Brentano held that all mental states have 'objects', and that this is what distinguishes the mental from the physical.

<sup>2</sup> There is an essay with this title in Clifford's *Lectures and Essays*, Vol. 2

begins by drawing attention to some important parallels between the philosophy of belief, on the one hand, and moral philosophy on the other. Such words as 'justifiable' and 'unjustifiable', phrases like 'have a right to', 'have no right to', are used when we are making judgments about someone's beliefs no less than when we are making judgments about his actions. Professor Chisholm brings this out by saying that 'adequate evidence' is an *appraisal* term, a term of 'epistemic appraisal', as 'right' and 'good' are terms of ethical appraisal. And he argues later (chap. 7) that the utterances in which we make our appraisals, whether epistemic or ethical, are neither true nor false. His view is, I think, that they express certain basic decisions to adopt such and such epistemic or ethical standards (standards of 'justifiability' whether for belief or for conduct) though he himself describes these decisions as 'convictions', a word he should not use, since convictions surely *are* true or false.

But just how far can the parallel between the two sorts of appraisal be pressed? Human actions are only subject to ethical appraisal in so far as they are voluntary. Within limits we can act as we choose. But can we believe as we choose? This is a rather difficult question, and two of the classical writers on the subject, Descartes and Hume, have given diametrically opposite answers to it.<sup>1</sup> Hume holds that belief is wholly involuntary. Descartes holds that we are free to give or withhold assent as we choose. Professor Chisholm has not discussed the question. But it is surely very relevant to his argument. If Hume is right in saying that belief is wholly involuntary, it will be difficult to speak of the 'ethics' of belief at all.

Nor has Professor Chisholm said anything about *degrees* of belief. The traditional view is that belief does have degrees, and they have been arranged in a series, from surmising or suspecting at the bottom end to complete conviction at the top end, with opinion somewhere in the middle. Perhaps Professor Chisholm wishes to reserve the term 'belief' for conviction, as some other writers have. If I think (have the opinion) that we shall get home in time for lunch, without being absolutely convinced of it, perhaps he would say that this is not an instance of belief at all. But if so we must criticise him for not discussing the appraisal of opinions and surmises as well as the appraisal of beliefs. Evidence which is adequate for opinion may be quite inadequate for conviction. The weather-forecast I hear on the wireless tonight may be quite adequate evidence for thinking ('opining') that there will be thundery showers tomorrow afternoon, but not for being completely convinced that there will. And similarly evidence which justifies surmising or suspecting that *p* may be inadequate for the opinion that *p*. It seems to me that Locke was right when he said that there are degrees of assent, and that in a reasonable man the degree of his assent varies with the strength of his evidence.

<sup>1</sup> Descartes *Meditations* (Meditation IV). Hume *Treatise of Human Nature*. Appendix

Professor Chisholm's own definition of 'adequate evidence' is this. He first introduces the undefined appraisal term 'more worthy of belief than'. Next he says that 'it is unreasonable for S to accept *h*' is equivalent to 'non-*h* is more worthy of S's belief than *h*'; and finally, that 'S has adequate evidence for *h*' is equivalent to 'it is unreasonable for S to accept non-*h*' (pp. 4-5). But what happens when there is no evidence either way, or when the evidence on either side is exceedingly slight and flimsy, e.g. the evidence for and against the proposition that the cat-population of Canterbury exceeded one hundred in the reign of King Ethelbert of Kent? The only reasonable course, surely, is suspense of judgment. This was the view of Clifford. But Professor Chisholm thinks that Clifford's view is too rigid. He says that we have a right to believe that a Republican will be President of the United States in 1975, because it would be unreasonable, on the present evidence, to believe that a Republican will *not* be President in that year (p. 9). We must agree, I think, that a man has a right to adopt this proposition just as a hypothesis or supposal. We must also agree that he has a *moral* right to believe it, in the sense that he will not be morally blameworthy if he does. But I cannot see that he has an 'epistemic' right to believe it, or that he would be reasonable if he believed it—at any rate if belief is regarded as a matter of all or none and is not allowed to have degrees. (Conceivably a man might have just enough evidence in 1958 for surmising or suspecting that a Republican will be President seventeen years later, but I think he would not have even that if we substituted 2025 for 1975.)

So much for the definition of the appraisal-term 'adequate evidence'. Can anything be said about the situations to which this appraisal-term is applied? This is the theme of chapter 3. Professor Chisholm points out that if we consider the analogous case of ethical appraisal, the actions which we appraise as right are always found to have 'right-making' characteristics of one sort or another, and these are themselves describable in ethically-neutral language, i.e. without the use of ethical appraisal terms. For example, the action was the saving of another person's life. Similarly, we may expect that any situation which we appraise as adequate evidence for someone's belief will have some 'evidence-bearing' characteristic or characteristics and that these will be describable in epistemically-neutral language. An evidence-bearing characteristic, or *mark* of evidence, can also be regarded as a criterion for applying the appraisal-term 'adequate evidence'. Can we find such criteria? This brings us to Professor Chisholm's theory of perception.

He maintains that one such criterion or mark of evidence is provided by certain sorts of appearing-situations. But first he draws a useful distinction between the comparative and the non-comparative use of appear-words (chap. 4). If someone says 'This looks blue', he may be using the appear-word 'look' in its comparative sense. In that case he means that the thing



looks as a blue thing might be expected to look in the present conditions of illumination, etc. But if the conditions of illumination are unusual it may very well be that in another (and presumably more fundamental) sense of the word 'look' the thing does not look blue at all; it may look green instead. This is the *non-comparative* sense of the word 'look'. The important difference between the two senses is that in a comparative appearing-statement the speaker is relying upon some extraneous information, e.g. upon some inductive generalisation about the way blue objects look in such and such conditions of illumination, or to percipients wearing various sorts of coloured spectacles. So appearing situations of this sort do not by themselves provide us with a mark of evidence. They are not independently evidence-bearing. But non-comparative appearing-situations come much nearer to being so. The only difficulty about them is that when someone says 'this thing looks blue' in the non-comparative sense he might be having a hallucination; in which case nothing would be appearing to him at all. In claiming, as he implicitly does, that he is *not* having a hallucination, he is relying on extraneous information concerning the physiological or psychological conditions in which his experience occurs. To get round this difficulty, Professor Chisholm suggests that he might use the verb 'to appear' in the passive. Instead of saying 'something appears blue to me' one might say 'I am appeared-to blue', or 'I am appeared-to in a way which is blue'. This statement is an independently evidence-bearing one. When we appraise it, we have to say that the speaker, provided he is speaking sincerely, does have adequate evidence for it, and in making it he is not relying upon any extraneous information (pp. 61-62).

Later on (chap. 8) Professor Chisholm replaces 'I am appeared to blue' by the less odd but more technical expression 'I sense blue'. He carefully explains, however, that he is not propounding any kind of sense-datum theory. Instead, it is what is sometimes called an 'internal accusative' or 'adverbial' theory. He is not saying, as the sense-datum philosophers do, that the sentient person is being aware of a blue *entity*, but rather that he is sensing in a blue *manner*, sensing 'bluecly' if we like to put it so.

But he also insists the such appearing-situations or sensing-situations are not the only evidence-bearing ones, as some Empiricist philosophers have thought they were (chap. 6). No combination of appearing-situations, however numerous, is by itself sufficient to provide adequate evidence for a physical object statement. (This point is further elaborated in the Appendix on Phenomenalism at the end of the book.) There is not only sensing or being appeared to. There is also what Professor Chisholm calls 'taking'—what some other people have called perceiving, as distinct from sensing. When I sense bluecly or am appeared-to in a blue manner, I 'take it that' I am sensing bluecly *with respect to* something, i.e. with respect to some physical object or event. And such 'sensible takings' ('perceptual takings' would

perhaps be a better phrase) are evidence-bearing situations. According to Professor Chisholm, they provide us with adequate evidence that there is indeed some physical object with respect to which I am sensing thus and thus. The word 'adequate' seems to have a slightly weakened meaning here. At an earlier stage of the argument it appeared to mean 'conclusive'. But the evidence which any one perceptual taking provides is not of course conclusive (otherwise there would be no such thing as hallucination). Professor Chisholm must mean, I think, that it is *good* evidence as far as it goes; and other perceptual takings may increase it, though he is not very explicit about the way they do so.

But how shall we define the crucial phrase 'sensing *with respect to* something'? Professor Chisholm holds that it must be defined in causal terms (chap. 10), and this contention is perhaps the most interesting idea in his book. When we sense blue and take ourselves to be sensing with respect to something (i.e. with respect to some physical object) we are accepting certain propositions about the cause of our sensing. Roughly, the propositions which we accept are (1) that there is some object which is stimulating one of our sensory receptors (2) that our sensing varies concomitantly with variations in the stimulating object. Professor Chisholm insists, however, that in accepting these propositions we are not making any kind of causal *inference* ('inferring physical objects from our sensations') as some philosophers have supposed we do. The propositions we accept are indeed causal ones. But we do not propound them to ourselves as an explanatory hypothesis to account for our sensing, and our 'acceptance' of them consists only in the fact that we should be very much surprised if they turned out to be false.

This causal analysis of 'sensing with respect to' raises some difficult questions which Professor Chisholm does not discuss. It has usually been thought that the 'perceptual takings' of the ordinary unsophisticated percipient have a Naïve Realist character. On this view, our ordinary assumption is that normal perception, or at any rate normal sight and touch, give us a direct knowledge of physical objects and their qualities. Consequently, when the ordinary percipient learns about the physical and physiological explanation of the causes of sensations (even in the relatively crude form which it took in the seventeenth century) he finds this explanation surprising and disconcerting. For it seems to show that perception is something quite different from what he had supposed it to be: not a way of becoming aware of physical objects and their qualities, but merely a way in which these objects affect us or operate upon us, causing us to have various sensations. And then we are led to wonder how we can possibly know that there are any physical objects at all, and many books are written on 'the problem of the external world'. We are also led to wonder how the physicists and physiologists can possibly have made these discoveries. For

they are, after all, discoveries about the physical world, including human sense-organs and brains which are of course part of it ; and they are presented to us as *empirical* discoveries, supported by perceptual evidence. But how can there be perceptual evidence for any proposition about the physical world, if perception consists only in being caused to have sensations ? When confronted with these awkward questions, most of us contrive to get round them by practising a kind of 'double-think'. In scientific contexts, or in technological ones (e.g. concerning the design of telescopes) we accept the physical and physiological explanation of the causes of our sensations, and think or speak in the technical language appropriate to it ; in everyday contexts—when driving a car, or sitting down in a chair—we continue to accept our original Naïve Realist assumptions. But if we happen to be philosophically inclined this expedient does not satisfy us, and we are likely to become either sceptics or phenomenologists.

Ever since the seventeenth century, philosophers have been greatly puzzled by these difficulties. How does Professor Chisholm propose to solve them ? I think his view is that they do not need to be solved, because they arise from a mistaken presupposition. Perception never had the Naïve Realist character which philosophers suppose it to have, not even in the most unsophisticated and unscientific percipients. Nobody ever accepted a non-causal theory of perception. So nobody has any ground for being disconcerted or puzzled when a causal theory of it is propounded by men of science. In principle the discoveries of physicists and physiologists about the causes of sensory stimulation are not 'news' to the ordinary percipient, though of course they are in detail. For he has always assumed, in a crude and vague way, no doubt, that something like what the physicists and physiologists now tell him is true. Perhaps we might put Professor Chisholm's view in this way : when the unscientific and unsophisticated person sees a tree, he takes the tree to be *doing* something to him (to be causing him to be 'appeared-to' in a certain way) ; and he takes it that if the tree were to alter in certain ways, e.g. if its branches were to be bent by a gust of wind, what it does to him would alter in concomitant ways. The causal theory of perception, so far from conflicting with our common sense view of the world, is itself just 'organised common sense', the systematisation and elaboration of something which is present from the first in our everyday perceptual attitude.

If we are to do justice to this doctrine, we must be perfectly clear about one point. Professor Chisholm is not just saying what every educated person would admit, that environmental objects do *in fact* stimulate our sensory receptors and that our sensings (our 'being appeared-to' in various ways) are *in fact* caused by such stimulations. He is going much further. He is saying that in perception we are taking ourselves to be so stimulated, and that such perceptual 'taking' in itself—and quite apart from scientific dis-



## REVIEWS

coveries—provides adequate evidence for the existence of such stimulus-producing objects. And consequently, so far as its general principles are concerned, though not its details, the causal explanation of the genesis of sensations is not just a scientific hypothesis, but a general formulation of the deliverances of perception itself. The physicists and physiologists are merely dotting the i's and crossing the t's of something which all percipients have always known, or at any rate have always had adequate evidence for believing.

Shall we accept Professor Chisholm's theory? We may certainly wish that we could. It gets rid of a tangle of problems which have worried epistemologists for centuries. The question is, however, whether it does justice to the phenomenology of perception, and especially the phenomenology of sight. Professor Chisholm, like many other philosophers, confines himself almost entirely to sight; nearly all his examples of 'appearings' are visual ones. But the fact is, I think, that his theory applies much better to touch. It is very plausible to say that in touch we take a physical object to be 'doing' something to us, causing us to have feelings of pressure and contact, and that we take these feelings to vary concomitantly with variations in the object or in its relations with our skin. Perhaps something similar could be said about hearing and smell. But can it be said about sight? The question, we must remember, is not about what is actually the case (is actually going on) when we see, but about what we perceptually take to be the case. I see a matchbox in front of me now. Its top looks yellowish and roughly rectangular. Am I taking it to be doing something to me, to be the remote cause of my being 'appeared-to' in this yellowish and more-or-less rectangular way? Assuming that I am a scientifically educated person, I should of course assent to these causal propositions if they happened to occur to me. But is the acceptance of them a part of the visual perception itself? I am strongly inclined to answer 'no'. It appears to me that what Professor Chisholm has done is to analyse visual perception on the model of touch; and it is by no means clear that the model applies.

H. H. PRICE

*Philosophy in the Mid-century: A Survey.* Edited by Raymond Klibansky. Florence—La Nuova Italia—editrice, 1958. Pp. xi + 336. Lire, 5,000.

THIS volume is one of four (of about 1,100 words in all) which are intended 'to provide a kind of general map for those who wish to look beyond familiar territory. . .'. On the whole the map here presented would seem to conform—topologically at least, if not always in scale—to the

territory. Most readers will as usual have their minor complaints to make, that some contributors have allowed too much space for their own views, that some extremely 'pure' parts of pure mathematics have been included, that some contributions are too technical or not technical enough, and so on. These and similar complaints would only suggest that the complainants expect too much from a survey of contemporary philosophy by contemporary philosophers. For a reviewer it seems best to limit himself to a survey of the survey. But even so he will very likely be adding some distortions of his own.

Logic and Philosophy of Science each take up about half the book. Although logic has to a large extent become an independent subject it retains its special relevance to philosophy. This is the topic of a brief and elegant preamble to the first half of the volume, by W. V. Quine. He makes two main points. First, quantification-theory, is 'finished neutral logic' and as such is a device for discovering 'whether a given theory involves commitments to objects of a given sort'. One might question the neutrality. In so far as the quantifiers—say of Quine's *Mathematical Logic*—are not mere abbreviations for certain quantifier-free contexts involving '&' and 'v'—the user of them is committed to the controversial ontological assumption of actually infinite ranges of objects. Quine's second point is that since general set-theories are *not* unique and neutral (which quantification theory is) the choice between alternative set-theories, is to be made in the light of 'the indirect systematic contribution which they make to the organizing of the empirical data of the natural sciences'. Here one might argue that Quine's pragmatism with respect to general set-theory is again a controversial philosophical position concerning modern logic and not a (neutral) consequence of it. That 'advances in pure logic can help to illuminate philosophical themes', however, will not be denied by anybody who has come in contact with Quine's writings.

The brief introductory essay is followed, first, by a solid and comprehensive historical survey by Canon Feys, divided into three parts—non-formalised logic, formalised logic, and philosophy of language. One could have wished for an appendix or a separate contribution from Feys on combinatory logic, after the fashion of some of the less historical and more expository essays of the volume. The survey is supplemented by two papers on the recent work of Polish logicians. One by Kotarbiński shows how logic has flourished in Poland since the downfall of Hitler and assures us that 'the divergences of opinion provoked by the contact between logic and dialectical materialism have been liquidated'. The other by Prior is a tribute to Łukasiewicz and a report on his work especially during the last years of his life. A survey by Putnam on elementary logic and the foundations of mathematics is primarily intended to exhibit the effect of new developments on elementary texts. It, however, also draws attention to

## REVIEWS

important new results, e.g. by Kreisel on the interpretation of non-finitist proofs—to name one result of philosophical significance.

One of the most interesting contributions (at least to one reader) is Beth's expository paper on semantics as a theory of reference, including a section of new results which extend semantic analysis to intuitionist logic as formalised by Heyting. This paper is followed by Heyting's lucid introduction to intuitionist mathematics and intuitionist philosophy of mathematics. Here, as elsewhere, Heyting mentions the disagreement between intuitionists 'about what is allowed in mathematics and what is not' and comments that it 'must not be taken too tragically'. But does the possibility of such disagreement not throw serious doubts on the intuitionist philosophy of mathematics as opposed to its mathematics?

An expository paper by Henkin on the Foundation of Mathematics presents this subject in the light of recent contributions by mathematical logicians. It is entirely free from the philosophy-is-bunk prejudice, the root of which lies too often in a confusion between philosophy of mathematics and some usually rather trivial part of mathematics; but it makes the reasonable demand that philosophers should acquaint themselves with new work in the 'Foundations'. Henkin's aim is to interpret technical results 'in more discursive terms' and is admirably achieved. Myhill's paper on the theory of recursive functions shows that Post's early predictions (1944) about the paramount importance of recursive number theory as a fundamental branch of pure mathematics were quite realistic.

De Finetti describes the present state of empirical and logical theories of probability and defends his own version of a subjective theory against two objections (i) that it is 'psychologistic' and (ii) that it presupposes the logical theory. He argues persuasively that 'everything that is of concrete value' in the logical and empirical theory 'finds its proper place and its natural explanation in the subjective interpretation'. (This theory cuts the Gordian knot of the problem of prior probabilities, since for it 'the initial probability is given by the initial opinion whatever it may be'.) The subject of Braithwaite's report is the central mathematical ideas of the theory of strategic games and their applications. Here, according to him, is the new mathematics that is needed for theoretical sociology and a new device for elucidating logical and ethical concepts. Recent work on the problem of induction is reviewed by Black. Although of opinion that the problem of induction arises from a confusion of thought, he gives a full (and fair) account of other points of view.

Turning now to the survey of the philosophy of science, this starts with a report by J. O. Wisdom covering publications on the methodology of science in English. Much of the recent work is related, or can very naturally be related, to Popper's writings and to the disagreements between Popper and Carnap or between Popper and Reichenbach. This is exactly what



## REVIEWS

Wisdom does. The main items of his report are Braithwaite's 'waste-paper-basket' view of probability hypotheses, the Popper-Carnap dispute on the logic of confirmation, and Braithwaite and Hutten on models. Operationalism and verification are still widely discussed—even the new points made being surrounded by an aura of *déjà vu*.

That the European methodology of science is altogether different from the Anglo-American variety is confirmed by Filiassi-Carcano's paper. There are, of course, exceptions such as Juhos and Dingler. Judging by the number of pages devoted to Gonseth's ideas he is the central continental figure in this branch of philosophy. His own paper—a *Vue d'ensemble*—of about seventy pages—is an exposition of his dialectical philosophy of science: For example, a tension between the aspects of formulation and experimentation confers an evolutionary character on every kind of inquiry including pure mathematics. The development of every theory proceeds in four phases. The conviction that these and other theses are true must arise from a detailed study of the theory and its development; and the present is not the occasion for either a reproduction or a criticism of Gonseth's account. It is at all events very well informed about the sciences which it seeks to illuminate by a 'dynamic' rather than a 'static' analysis. Destouches discusses the relations between modern physics and philosophy. The main stimulus here has come from a revival of some older views of de Broglie, by Bohm and Vigier and from some new 'unorthodox' views by Bohm—all concerning the interpretation and further development of quantum-mechanics. There follow brief accounts on parallel developments in Belgium (Renoirte, Klibansky), Denmark (Petersen), Spain (París), U.S.A. (Lenzen, Feigl, and Maxwell), Italy (Somenzi), Holland (van Melsen), U.S.S.R. (Omelianovski) and two somewhat longer essays, one by Mercier on developments in Germany, Austria, and Switzerland, and one by Dingle on developments in England.

In the penultimate chapter Niels Bohr describes his well known (and still dominant) philosophy of causality and complementarity. The volume ends with a paper by van der Klaauw on biology and philosophy, which shows that all the old theories are still alive and that all points of view still have their defenders.

This very brief synopsis will perhaps show what roughly a reader is to expect from the volume. Future historians of philosophy are sure to find it of great value; the contemporary general reader will wish to read more about some of the subjects discussed, and even some experts will be made to look up quickly some original paper which has so far escaped them. Editor and contributors alike deserve the congratulations of everyone.

S. KÖRNER

## REVIEWS

*Truth and Denotation, A Study in Semantical Theory.* By R. M. Martin.  
Routledge and Kegan Paul, London, 1958. Pp. xii + 303. 42s.

ANYONE who agrees with the author that 'the methodology of the sciences is now pre-eminently concerned with the semantics of scientific languages' will find here a thorough exposition of the whole subject. The first chapter is devoted to a discussion of the merits of the construction and study of formalised languages as a method of dealing with philosophical problems, including the methodology of natural science, as well as to an introductory exposition of the process of formalisation itself in its syntactical and semantical aspects. In Chapter II an account is given of first-order languages, i.e. those with a very simple and elementary structure because only individual variables are admitted. The essentials of sentential calculus, quantification theory, and the theory of identity are outlined. This is immediately followed in Chapter III by a detailed account of first-order syntax. Semantical notions are reached in Chapter IV in which a semantical metalanguage is constructed, based on the notion of *multiple denotation*. Among the various ways in which 'denotation' can be used, multiple denotation is that according to which 'a class or property word is regarded as denoting the members of the class in question'. 'What are ordinarily regarded as class names thus come to denote severally the members of the class but not the class itself.' The author shows that denotation interpreted in this way is a very powerful semantical relation, in terms of which all other semantical notions that are required can be defined. On this basis several alternative definitions of 'true' are given in Chapter V and their adequacy proved. The connection between truth and consistency and decidability is discussed and the semantical antinomies are considered. On pages 124-125 the distinction between providing a *criterion* of truth and providing an *analysis* of this notion is pointed out; semantics is concerned with the latter, a point which is frequently overlooked. Chapter VI is devoted to the study of the non-logical part of two important first-order languages: one based on the Zermelo set theory, and one based upon Russell's simplified theory of types. Although the word 'relation' does not occur in the index relations are dealt with on page 155 under set theory. In Chapter VII alternatives to the relation of multiple denotation as a fundamental undefined semantical notion are considered, namely the two-termed relations of satisfaction, determination and designation. Satisfaction has already been mentioned on page 107 where it is defined with the help of multiple denotation. Here it is taken as primitive and multiple denotation defined with its help. The equivalence of the two definitions is demonstrated. Certain limitations which apply to designation and determination but not to denotation and satisfaction are pointed out.

The semantical metalanguages so far developed up to this point have closely followed the pattern of those studied by Tarski and Carnap. They

have shared the common feature of containing a translation of the object language about which they speak. Professor Martin calls these *translational* metalanguages. In Chapters VIII and IX we reach the most original part of the book, where the author shows how *non-translational* semantical metalanguages can be constructed using 'comprehension' as the sole semantical primitive. One predicate constant *comprehends* another if it denotes everything that the other denotes (the term was used in essentially the same sense by Hobbes in 1653). It is shown that 'the general theory of comprehension consists essentially of elementary syntax together with a Boolean algebra of predicate constants' (p. 192). Four definitions of 'true' are offered. Attention is called to the characteristic feature of non-translational semantics that the requirement of adequacy for its truth definitions must be abandoned. This is because the statement of the requirement of adequacy presupposes that within the semantical metalanguage we have both the expressions of the object language *and* their structural descriptions; and this condition is, by definition, not satisfied by non-translational metalanguages. Professor Martin points out that nevertheless the meaning of the one primitive notion 'comprehension' is such that the resulting truth-concept 'is essentially the same as one within a formalism in which the requirement of adequacy can be stated and proved in full generality'. Moreover, although the adequacy cannot be proved it is not denied but rests on intuitive considerations concerning the precise meaning of 'comprehension'.

The object-languages for which the preceding metalanguages have been constructed have contained no primitive individual or functional constants. Chapter X is devoted to the changes which are needed in order to render these metalanguages applicable to object languages which do contain such constants. Another feature of the metalanguages of preceding chapters is that they all regard the expressions of the object-language as sign-designs or shapes; Chapters XI and XII deal with the problems connected with treating such expressions as sign-events or inscriptions. The book ends with a most interesting discussion, in Chapter XIII, of what the author calls *first-order constructivism*. In an earlier chapter (p. 61) first-order languages in the narrow sense are distinguished from first-order languages in a wider sense. By the former are understood languages in which the domain of individuals is either finite or denumerably infinite; by the latter are meant those in which the domain of individuals is non-denumerable. Professor Martin then formulates the general methodological maxim that *wherever there is a choice a first-order language in the narrower sense is to be preferred to one in the wider sense*. The reasons for adopting this maxim are elaborated in Chapter XIII. By 'first-order constructionalism' Martin means the philosophical point of view underlying the general methodological maxim. He discusses at length and in a most interesting way its relation to nominalism and realism. The object languages discussed in this book have not been confined in



## REVIEWS

accordance with the methodological maxim because that would be to restrict their scope too much: 'science and mathematics are shot through and through with concepts resting fundamentally upon the non-denumerable' (p. 264). But the translational semantical metalanguages here developed are of first order in the narrow sense provided the corresponding object-language is also of first order in the narrow sense. On the other hand, the non-translational metalanguage is a denumerable first-order semantical metalanguage irrespective of whether this is the case for its object-language. It is claimed and shown that first-order constructivism occupies a middle position between nominalism and realism (Platonism). The relative merits of non-translational as opposed to translational semantics are discussed on page 273. This book can be warmly recommended to anyone who requires a thorough discussion of the tasks of semantics.

J. H. WOODGER

*British Philosophy in the Mid-Century*, edited by C. A. Mace.

George Allen and Unwin Ltd., London, 1957. Pp. 396. 30s.

THE essays in this volume<sup>1</sup> have their origin in a course of lectures organised by the British Council in the summer of 1953 at Peterhouse College, Cambridge, where the Faculty of Moral Sciences was responsible for the programme and nominated Miss Margaret Masterman and Dr Theodore Redpath as joint Directors of Studies. The course was attended by professors, lecturers, and teachers of philosophy from abroad.

Although, naturally, most lecturers were Cambridge philosophers, contributions were invited from other Universities as well, and thus the volume seems to present a fair picture of contemporary British philosophy, as far as this matter can be judged by a foreign reviewer.

I believe that most readers will enjoy the volume as a whole; but the contributions by Braithwaite, Popper, and Bondi are more properly in the field of this *Journal*.

Popper, in his 'personal report', states and explains his *problem of demarcation* (which is concerned with the demarcation between the statements

<sup>1</sup> C. D. Broad, 'The Local Historical Background of Contemporary Cambridge Philosophy'; A. C. Ewing, 'Recent Developments in British Ethical Thought'; C. A. Mace, 'Some Trends in the Philosophy of Mind'; S. Körner, 'Some Types of Philosophical Thinking'; R. B. Braithwaite, 'Probability and Induction'; K. R. Popper, 'Philosophy of Science: A Personal Report'; H. Bondi, 'Some Philosophical Problems of Cosmology'; G. E. Moore, 'Visual Sense-Data'; A. J. Ayer, 'Perception'; G. Ryle, 'The Theory of Meaning'; S. Hampshire, 'The Interpretation of Language: Words and Concepts'; M. Masterman, 'Metaphysical and Ideographic Language'; Th. Redpath, 'Some Problems of Modern Aesthetics'.

## REVIEWS

of the empirical sciences, and all other statements, religious, metaphysical, or simply pseudo-scientific), the *criterion of falsifiability* (testability, or refutability) as a solution of this problem, his criticism of Wittgenstein's criterion of meaningfulness, and the misunderstandings with which these ideas have met. He then presents his objections to Hume's psychological theory of induction, which he replaces by the following view. Without waiting, passively, for repetitions to impose regularities upon us, we actively try to impose regularities upon the world. Without waiting for premises, we jump to conclusions which, of course, may have to be discarded later, should observation show that they were erroneous. As Popper realised later, the problem of demarcation and the problem of induction are in a sense one. Both can be solved if we observe that the method of science is that of criticism, i.e. of attempted falsifications.

Apart from details, Popper's discussion seems to be convincing. Perhaps too much emphasis is laid on the distinction between his problem as a problem of demarcation and the problem of many others as a problem of meaning; for in many cases, the other philosophers were interested in *empirical* meaningfulness. In such cases, the difference with Popper's approach seems to be merely terminological.

For instance, Braithwaite's contention (p. 142) 'that . . . the meaning of statistical general statements is given by a series of empirical rejection tests, all of which are provisional and subject to revision', seems to be fully consistent with Popper's proposed solution of the problem of demarcation.

Another case in point is found in Bondi's essay, where (p. 196) it is stated, in connection with a certain way of constructing a model of the universe, that the 'observational comparison that can now be made makes a disproof of the postulates possible and so gives them scientific status.'

Falsifiability as a criterion for meaningfulness is not restricted to empirical statements but can be extended to statements in the formal sciences as well; this is illustrated by Kreisel's no-counter example interpretation for classical logic and mathematics (*cf.* this *Journal*, 4, pp. 107-129).

To conclude, I wish to express my admiration for the way in which the volume has been produced.

E. W. BETH

*A Natural Science of Society.* By A. R. Radcliffe-Brown. Foreword by Fred Eggan.

The Free Press, Glencoe, Illinois, 1957. Pp. xii + 156. \$3.50.

In 1937 the late Professor Radcliffe-Brown gave a series of talks to a seminar in Chicago University. He spoke from brief notes, and a verbatim record

## REVIEWS

was kept. He had intended to re-write this for publication, but died without doing so. His lectures are now published as spoken. Naturally they contain some roughness and looseness of expression; but they are remarkable for their conciseness and economy. What he had to say then is still interesting and controversial today.

He argued that a unified natural science of society is possible, though it is as yet in its infancy. This science is social anthropology, which studies (or should eventually study) the whole social system of which economics, political science, etc., study only aspects.

The notion of a *system* is the key idea in this book. The only really relevant point which emerges from Radcliffe-Brown's animadversions on science in general is that what science studies is systems. At first, he rather suggests that a natural system is marked off from its surroundings and given its unity, not so much by nature as by the scientist. A system is something which has been '*conceptually isolated*'—'*we perform a dichotomy*' (my italics). But this tends to give way to the idea that systems as such exist 'out there', independently of our organising concepts. It is, he claims, the objective coherence of the system which separates it off. (His favourite physical example is the solar system.) This idea has an important influence on his social theory.

He claims that to the different kinds of system—mechanical systems, chemical systems, biological systems, social systems—correspond the different sciences: first we perceive the various systems, then we study them. I think we need scientific theory *before* we can perceive that a number of discrete things are all members of one system and before we can perceive that a number of seemingly disparate systems are variegated examples of one kind of system. This criticism rebuts Radcliffe-Brown's contention that the *first* task of a science of society is a taxonomic classification of all the kinds of social system there are. A classification will be the more jejune, unwieldy and fruitless the less theory-impregnated it is.

Nor do I feel much enthusiasm for his Aristotelian belief that, having classified and described all these social systems, we should, by comparison, discover the essential, residual factors common to them all. Radcliffe-Brown mentions as examples of such universal factors: the existence of some moral system together with some notion of justice; and social 'coaptation', or the 'fitting together' of various people's behaviour (though this is really part of his definition of 'social system').

After classification and description come the problems: How do social systems persist and change? Radcliffe-Brown's notion of a living, autonomous social system exerts a strong influence on the way in which he would answer this question. He speaks (as anthropologists concerned with closed communities tend to speak) of a particular society as 'a perfectly concrete discrete thing'. Social systems, he says, can be observed. They have a



## REVIEWS

self-equilibrating tendency. Social usages have a function, that is, they are necessary for the persistence of the social structure.

In his *Structure and Function in Primitive Society* he held that historical explanations and functional explanations 'do not conflict, but supplement one another' (p. 186). This would be so if the historical anthropologist is regarded as describing what antecedent factors have brought a certain social usage into existence while the functional anthropologist describes the beneficial consequences of that usage for society. But in the book under review Radcliffe-Brown advanced in the intimacy of the seminar-room the less diplomatic view that the historical approach is 'a real obstacle to the development of social science'. This is a passage he would no doubt have expanded and made unambiguous if he had been able to prepare the book for publication. I read into it, perhaps mistakenly, the suggestion that it is not for the historian but for the functional anthropologist to explain (not merely the beneficial effects of a social usage but) *why* the usage exists. This is tacitly to attribute to a social system a capacity for developing and controlling its component parts according to its overall needs. One can hardly attribute such foresight and power to a social system if one recognises (as Radcliffe-Brown at times recognised) that social life never presents us with a nicely demarcated, autonomous, and comprehensive social system, but only with people forming and re-forming into shifting and overlapping and often competing sub-systems.

J. W. N. WATKINS

*Theories of the Universe from Babylonian Myth to Modern Science.* Edited by M. K. Munitz.

The Free Press, Glencoe, Illinois, 1957. Pp. x + 437. \$6.50.

NEVER in its long history has cosmology been the subject of so much intense and informed speculative activity as at the present time. Giant telescopes now serve to probe hitherto inaccessible depths of space, while interpretative 'models', grounded in the concepts of modern physics, divide the allegiance of astronomers. Such recent advances should be viewed against the background of earlier efforts and achievements; but the writings of the pioneers in this field are not readily available nor always intelligible to the student, and the need has long been felt for a source book designed to set the successive phases of cosmology in historical perspective. This want has now been supplied in generous measure by Professor Munitz of New York University who has reproduced in a single volume some thirty extensive extracts from cosmological classics and commentaries.

The selected readings are grouped into four sections corresponding to four main stages in the development of cosmology. The first section illus-

## REVIEWS

trates the transition from the pure mythology of *Enuma Elish*, the ' Babylonian Genesis ', to the crude but rationalistic world-views of the pre-Socratic philosophers. In the absence of surviving texts, this phase is largely represented by extracts from the standard works of T. Gomperz, F. M. Cornford and Thorkild Jacobsen. The doctrine of a geocentric, finite universe, constituting the second phase, is set forth in the classic words of Plato, Aristotle, and Ptolemy, with a chapter on medieval cosmology reprinted from J. L. E. Dreyer's *Planetary Systems*. The third period is conceived as extending from the end of the Middle Ages to the beginning of the present century ; its great names include those of Copernicus, Galileo, Newton, and Herschel, and it is marked by cosmologically significant discoveries in the field of observation and by theoretical trials of alternative possibilities on the conceptual side. It prepared the way for the current phase, marked by the extension of the field to the realm of the nebulae, the employment of sophisticated mathematical techniques and the formulation of hypothetical generalisations such as the doctrines of ' continuous creation ' and of the ' expanding universe '. The material illustrating this latest period and occupying more than a third of the book is extracted from the writings of A. Einstein, E. P. Hubble, W. de Sitter, A. S. Eddington, G. Lemaître, E. A. Milne, H. P. Robertson, G. Gamow, H. Bondi, D.W. Sciama and F. Hoyle. It has been selected from the less technical works of these writers so as to be generally intelligible to the non-specialist. The several sections are prefaced with essays introducing the selected classics and together constituting the outline of a continuous history of cosmology. A classified Bibliography concludes the work.

The selections here reproduced should be of particular interest to students of the philosophy of science as affording ample material for the analysis of cosmological concepts and methods. In the Editor's view, ' the study of the " logic " of cosmology, as distinguished from the pursuit of cosmological inquiry itself, is both a critical examination of the procedures involved in obtaining knowledge of the universe and an analysis of the meaning or meanings of the concept " universe " ' (p. vii). Such explorations will be greatly facilitated by the study of the texts here brought together for the first time.

In the diagram on page 166 the inscription ' Mars's Orbit ' should read ' Moon's Orbit ' ; and a systematic error in the Index has increased by three or four the page numbers of all the sixty-six entries relating to the Bibliography.

A. ARMITAGE

## REVIEWS

*The Appreciation of Ancient and Medieval Science during the Renaissance (1450-1600).* By George Sarton.

University of Pennsylvania Press, Philadelphia, 1955. Pp. xviii + 234. 40s.

*Six Wings. Men of Science in the Renaissance.* By George Sarton.

Indiana University Press, Bloomington, 1957. Pp. xvi + 318. \$6.75.

THESE two volumes are the last published works of the late Dr George Sarton, who died on 22 March 1956, before the second of them had gone through the press. It is not my intention to review them critically, but it is appropriate that a short notice should be published in this *Journal*. Dr Sarton was a man of facts rather than ideas. His most substantial contribution was to the bibliography of early science; he did not work with the philosophical and analytical approach to the history of science such as is now, in the hands of younger scholars, throwing so much light on the development and character of scientific thinking. A hard critic might even say that Sarton's approach could easily have killed the study of the history of science, which breathes through ideas, by suffocating it beneath the mountain of uninterpreted and unrelated facts which he spent a lifetime in collecting. But such a criticism would be too extreme. Now that what was bad in his influence no longer threatens, unhappily removed by his too-early death, it is possible to see that as a collector of bibliographical, biographical, and scientific facts he did work that is invaluable and was sometimes inspired. His large *Introduction to the History of Science* is an indispensable guide for any student of ancient and medieval science, and the present volumes give some idea of what the *Introduction* might have been had Sarton been able to carry it beyond the fourteenth century.

The first of these volumes, based on lectures given as Rosenbach Fellow in Bibliography in Philadelphia in 1953, is an account of the knowledge, editions, and influence of ancient and medieval scientific writings in the late fifteenth and sixteenth centuries. Three main subjects are discussed, medicine, natural history, and mathematics. The value of the book is that it gives in compact form the evidence showing just what difference classical scholarship and the editions and translations of ancient authors made to the scientific learning of the renaissance period, how medieval authors continued to be read and their works printed, and how medieval translations of Greek scientific writings were used and revised for renaissance editions. Renaissance science was dominated by philology, just as medieval science was dominated by metaphysics and theology, but the dominance in both cases was exerted through the same Greek forms of thought. When, in the seventeenth century, science was finally set on its feet as an independent branch of learning, this was done, as the biographies of such innovators as



## REVIEWS

Galileo, Francis Bacon, and Descartes show clearly, not by starting *de novo* (who has ever done that ?) but by the transcending current doctrines. In this detailed account of the scientific books available down to the beginning of the seventeenth century, Dr Sarton has given us a concrete picture of the sources of those doctrines.

The second volume under review, the Patten Lectures given at Indiana University in 1955, covers much the same period as the first in terms of the men of science rather than the books they read. The scientific subjects discussed are exploration and education, mathematics and astronomy, physics, chemistry and technology, natural history, anatomy and medicine, and the relations between art and science as exemplified by the work of Leonardo da Vinci. The account is plainly factual, and full of fascinating out-of-the-way information: an example is that Don Juan of Austria used for his strategy at the battle of Lepanto (7 October 1571) a detailed forecast of the weather (which happily turned out as predicted) made by the mathematician Francesco Maurolico. Together these two volumes give a concrete picture of science in the sixteenth century. The appetite to which they appeal is that which Leibniz described when he spoke of the 'thrill of learning singular things'. They have no philosophical interest and they do not discuss the architecture of scientific thinking on the larger theoretical issues that begin to arise in this period. Thus they do not seem to me to make the most illuminating approach that can be made to the history of science. But it is valuable to be given so many useful and interesting facts and fitting that through these last volumes we should salute the memory of a devoted pioneer.

A. C. CROMBIE

## ANNOUNCEMENT

### 1960 INTERNATIONAL CONGRESS FOR LOGIC, METHODOLOGY AND PHILOSOPHY OF SCIENCE

An International Congress for Logic, Methodology and Philosophy of Science will be held at Stanford University, Stanford, California, U.S.A., from 24th August to 2nd September, 1960, under the auspices of the *International Union for History and Philosophy of Science*.

The proceedings of the Congress will be organised in eleven sections, and will consist of a number of invited addresses, in addition to brief contributed papers. The closing date for submission of abstracts of contributed papers is 1st March, 1960.

Information about membership fees and other details of the Congress may be obtained by writing Professor Patrick Suppes, Serra House, Stanford University, California, U.S.A.

## ABSTRACTS

*Philosophy of Science*, 1958, 25, No. 1

### M. Brodbeck, 'Methodological Individualisms: Definition and Reduction'

Two meanings of 'methodological individualism' must be distinguished: (1) the view that there are no undefinable group concepts and (2) the view that the laws of the group sciences are in principle reducible to those about individuals. The former is a denial of descriptive emergence; the latter, a denial that there are any logical grounds for belief in explanatory emergence. The two views are logically independent. The denial of the possibility of group laws, as by J. W. N. Watkins, confuses these two issues. Only the denial of descriptive emergence is locally required by empiricism. Explanatory emergency is a matter of fact, dependent upon the existence of appropriate composition laws about individuals from which group behaviour may be derived. A distinction between perfect and imperfect knowledge of society is explicated. The former is logically possible, though neither plausible nor probable.

### W. Mikulak, 'Soviet Philosophic-Cosmological Thought'

Soviet philosophic assumptions related to the problems of modern cosmology, the approaches and solutions of the Soviet astronomers respecting these problems, and the extent to which these approaches and philosophic assumptions conform to the Party line formulated by Soviet Communist dialectical materialists for Soviet astronomers are examined. Russian cosmologists operate within the framework of the official philosophical world outlook of the Soviet state. Because of this framework the Soviet astronomers have rejected the following Western cosmological developments: the relativist finite but expanding universe, the relativist 'geometrization' of the force of gravity, the finite time-scale, the use of extrapolation in studying the universe as a whole, the creation of matter out of nothing, and the heat death of the universe. In a positive sense they have attempted to construct cosmological theories satisfying a universe infinite in space and duration in conformance with Marxist-Leninist philosophy.

### M. J. Swartz, 'On History and Science in Anthropology'

A review of the main positions of Anthropologists on the nature of history and the use of historical materials leads to classifying their positions into those who hold history to be the unfolding of similar events and who use historical material to show the presence of a force postulated on *a priori* grounds; those conceiving of history as a series of unique events and use historical data to explain cultural phenomena; and those viewing history as statements of ordered phenomena and using these statements for description rather than explanation. Whatever the view taken, general statements enter into the selection and evaluation of the data considered and generalities are involved, regardless of the use of historical materials. The distinction between history and science based on the use of general principles is held to be specious.

## ABSTRACTS

### N. Rescher, 'A Theory of Evidence'

Three evidence-concepts are explicated and analysed:

(1) The statement *q* is *evidentially presumptive* for the statement *p* if taking *q* as hypothesis renders *p* more likely than not, i.e. more likely than its negation;

(2) *q* is *supporting evidence* for *p* if, given *q* as hypothesis, *p* is more likely than before, i.e. more likely *a posteriori* than it was *a priori*; and finally

(3) *q* is *confirming evidence* for *p* if both of the preceding conditions obtain. A formal theory of these concepts is built up: measures of *degree of evidential presumption* and of *degree of evidential support* are introduced, and the formal logic of *rules of evidence* is considered for each of the three evidence concepts. The relation of the present treatment to previous discussions of the theory of evidence, particularly in the work of Carnap and of Hempel, is considered. The implications which may be drawn from a formal theory of evidence respecting the epistemology of disciplines, such as the social sciences where one must operate with *evidence* proper and leave behind the secure ground of *proof*, are examined.

### V. Hinshaw, Jr., 'The Objectivity of History'

Toynbee's variant of historical relativism and Mannheim's historicism are examined and both found to confuse epistemological questions concerning the possibility of historical knowledge with issues about the reliability of such knowledge in the light of the historian's selections and evaluations. Dewey and Mead are likewise seen to mix these questions in their overemphasis on the present and denial of a real past. Paralleling positivist treatment of philosophy of physical science, a conception of the methodology of empirical historiography is introduced to show that, as scientist, the historian simply takes for granted our knowledge of the past and can only be concerned with reliability. The author considers Mandelbaum's recent warning against reduction or integration of the social sciences, and suggests that a philosophy of history must, if it is to merit serious consideration, 'square itself' with the findings of the methodology of empirical historiography. A plea is again made for specification of factual issues involved and for a separation of these from results of methodological analysis and from questions of philosophy proper.

*Philosophy of Science*, 1958, 25, No. 2

### R. S. Hartman, 'Value, Fact and Science'

The parallelism between a science of facts and one of values, in much of contemporary literature, is obscured by the confusion *between the value of the scientific method and value as an object of this method*. Fact and value can both be subject matters of science, provided 'science' is regarded as a *method applicable to any subject matter whatsoever*.

The nature of science is to break down secondary or sense properties of any phenomenon—'fact', 'value,' etc.—into primary properties and use the latter as elements to reconstruct a formal frame of reference. The procedure of Galileo, on the one hand, and a possible 'Galilean' procedure in value theory, on the other, are investigated in detail, and the results compared with the opposite results of E.W. Hall, *Modern Science and Human values*.



## ABSTRACTS

### B. Mayo, 'The Incongruity of Counterparts'

(i) When an object is turned in space through  $180^\circ$ , it undergoes reversal in two dimensions (those perpendicular to the axis of semi-rotation).

(ii) When an object is reflected in a plane mirror, its image is reversed in one direction only. An object and its mirror-image are examples of *counterparts*, as are right- and left-hand gloves, etc.

(iii) Normally when an object is viewed together with its mirror-image, the incongruity is such that the image is 'back-to-front' with respect to the original.

(iv) Yet when we view the reflections of certain objects, such as our own faces, or printed characters, the incongruity is a *left-to-right* reversal.

The discrepancy between (iii) and (iv) is due to a combination of at least four factors whose effect can be assessed by imagining them absent or varied, but they are in fact so elemental and pervasive that a considerable imaginative effort is required.

The full explanation of the facts calls for a conjunction of surprisingly widely separated disciplines' geometry, optics and some elementary sociological, anatomical and psychological observations. A general result is given.

### R. Handy, 'Philosophy's Neglect of the Social Sciences'

Since philosophising is one form of human behaviour, and since the social sciences are discovering reliable knowledge about human behaviour, their findings and hypotheses are relevant to the philosophic enterprise. Social science factors may partially determine the philosopher's conception of his field, his choice of methodology, and his belief as to what problems are important. The sciences have often taken as part of their province problems which once were thought to be primarily philosophic, and it is suggested that this may now be happening with the social sciences. Since some philosophic theses are partially based on generalizations about human behaviour, certain philosophic doctrines may become highly unpalatable as knowledge about that behaviour advances. This is illustrated by recent attempts to relate the social sciences to value theory, metaphysics, and methodology.

### H. Putnam, 'Formalization of the Concept "About"'

The possibility of formalising the semantical relation: 'statement S is about class C' is discussed. In terms of Carnap's notion of 'amount of information' ( $\text{inf}(S)$ ), one can define a relative notion, 'the amount of information S gives concerning C' ( $\text{inf}(S, C)$ ). ' $\text{inf}(S, C) = \text{inf}(S)$ ,' i.e. 'the amount of information S gives about C is equal to the total content of S,' is then proposed as a definition of the concept 'S is strictly about C'. This notion is compared with the traditional one (a categorical proposition is 'about' the class corresponding to the subject term), and it is shown that the formal concept is in accord with the traditional usage, to this extent at least. A possible application to the 'ordering' of scientific disciplines, suggested by Dr Paul Oppenheim, is also briefly described.

### H. G. Apostle, 'Methodological Superiority of Aristotle over Euclid'

In his *Elements*, Euclid presents the pure mathematical sciences in the following order: Plane geometry (Books 1, 2, 3, 4), general geometry (Book 5), plane geometry (Book 6), arithmetic (Books 7, 8, 9), general geometry and plane geometry (Book 10),

## ABSTRACTS

and solid geometry (Books 11, 12, 13). This is neither Plato's nor Aristotle's order. Aristotle's order proceeds from the general to the specific, from the prior to the posterior in definition. Euclid was not well aware of and so did not use effectively the principles of unifying a science which were available then. Methodologically, Aristotle is closer to modern tendencies of unifying mathematics.

### *Philosophy of Science*, 1958, 25, No. 3

J. K. Senior, 'The Vernacular of the Laboratory'

An attempt is made to characterise the dialect ordinarily used by laboratory workers and to contrast it with the more formal linguistic systems devised by logicians and philosophers of science.

W. T. Fontaine, 'The Means-End Relation and its Bearing Upon Cross-Cultural Ethical Agreement'

The controversy between 'radical' and 'modified' ethical relativists cannot be settled by citing the failure of the latter to justify universally valid values. The demand increases for clarification of the meaning of validity and it might prove more rewarding to approach the problem from the point of view of the extent of cross-cultural ethical agreement.

Both modified and radical relativism admit cross-cultural agreement in *belief*. Thus, for both there may be cross-cultural agreement on means in the sense of an effective instrument. Since an effective means may become an extrinsic value or *approved* means and, thus, later an intrinsic value or end, it seems plausible that individuals of different cultures, under the stress of some new, common danger may agree upon an effective means. As a result of success, they may come to approve this means as extrinsically valuable.

A. Bachem, 'Ethics and Esthetics on a Biological Basis'

Through the complementarity between psychological and physiological states, the scope of the happiness principle (as presented by John Stuart Mill and others) is widened to an utilitarian principle of global health. This ethical system is thus developed on a biological basis and aims, not only to increase the individual sense of well-being, but mainly to increase the social usefulness of the individual. The educational means of achieving individual happiness and health should be based on facilitation rather than inhibition.

L. Skarsgård, 'Some Remarks on the Logic of Explanation'

In 'Studies in the Logic of Explanation', *Philosophy of Science*, 15, 1948, Hempel and Oppenheim present a model of causal explanation, M for short. This is described and distinguished from the level of genuine explanations which function in historical discourse; 'G' will stand for 'genuine explanation(s)'. Four differences between M and G are commented on, one of which is that probability implications hold from explanantia to explananda in G, whereas a logical implication holds between the corresponding parts of M.

Model 1 construction is an indispensable part of any thorough investigation of instances of G, whenever the task is to make clear their logical and semantical relations;

## ABSTRACTS

and that a set of models should be a constituent part of a coherent theory of historical explanation.

H. V. Stopes-Roe, 'Some considerations concerning "Interpretative Systems"'

The central notion of this paper is the 'generalised reduction sentence'. This is the obvious simplest generalisation of Carnap's reduction sentences, generalised to admit the introduction of more than one new term at a time. It is shown that a wide range of interpretative systems can be re-expressed in terms of generalised reduction sentences. Recursive predicates and functions can be thus re-expressed, and such peculiar relations as Heisenberg's uncertainty principle appear more naturally in this form than in the usual.

When interpretative systems are expressed within the lower functional logic the only bar to re-expression in reductive form is essential use of existential quantification; and so far as practice in natural science is concerned, it appears possible that the only significant application of existential quantification is in the introduction of 'unobservable objects'. Subject to the latter surmise, it follows that any scientifically useful interpretative system which is based on the lower predicate logic (without other restriction) is either equivalent to a set of generalised reduction sentences, or it is introducing 'unobservable objects'.

J. H. Kultgen, 'Philosophic Conceptions in Mendeleev's *Principles of Chemistry*'

Dmitri Mendeleev's conceptions of nature, natural science and the science of chemistry is reconstructed from remarks in his general text, the *Principles of Chemistry*. He generally speaks of a nature composed of discrete, permanent individual substances, to be physically isolated by man and described in terms of their properties relative to his sense organs. But certain remarks suggest that Mendeleev may have conceived of the ultimate character of nature as unknowable, that scientific concepts represent certain intelligible characteristics of the unknowable matrix, discovered through devices such as measurement and analogy to macroscopic events, and that the aim of science is to organise such concepts into the simplest possible system.

Shuntaro Itō, 'Biologische Erkenntnis und Moderne Physik'

Modern science started as physics in seventeenth century, being established by Galileo, Newton, etc. The methodological principle of this classical science is most clearly expressed in Cartesian philosophy. Mechanism had so much success in analysis of nature that biology had submitted to the mechanistic ideas until the end of the nineteenth century. The principal elements of the mechanistic ideas implied in classical physics has been destroyed in the development of contemporary physics. The problem of life should be reconsidered in a new light free from the traditional mechanistic view and have its own unique standpoint.

J. L. McKnight, 'An Extended Latency Interpretation of Quantum Mechanical Measurement'

A set of criteria is developed for an adequate interpretation of measurement in quantum mechanics. They are founded on traditional epistemology and metaphysics. An epistemological scheme similar to that of Northrop and Margenau is developed



## ABSTRACTS

which will fulfil these criteria. On this basis it is proposed that variables such as position, momentum and the like are not uniquely possessed by quantum mechanical systems. Between acts of measurement it would contradict quantum theory to ascribe definite values to the classical observables of the system. The observables are latent in this strong sense and appear only during the measuring process. This interpretation is applied to several examples and used to explain the Bohr principle of complementarity.

### W. Kent, 'Scientific Naming'

Lavoisier and Faraday, two of our greatest scientific namegivers, are examples of a common discrepancy between a theoretical respect for precision and caution in naming, and an actual vagueness and uncontrollability. Both neglected the fact that the meaning of words, perhaps even especially in science, are always plastic, and that metaphor and vagueness are indispensable for originating and modifying scientific meaning. To take a word as having a precise meaning kills it as an instrument of scientific progress.

### W. S. Wiedorn, Jr., 'Method in Research in Psychiatry: Implications for the Philosophy of Science'

An inquiry into the form of operations necessitated in research in psychiatry reveals differences between such operations and those found in sciences based on a physical model, including those biological or psychological sciences based more or less on a physical model. Participant observation, dictated by the nature of the phenomena studied, was described as the prime tool of research in psychiatry. The mutual modification of the observing person and the observed person in the matrix of interpersonal participancy suggests that sciences founded on that type of method be considered participant systems sciences.

## ERRATA

In the recently published list of members Professor Dingle should have been given as Professor Herbert Dingle, and, of course, an F indicating Foundation member should also have appeared beside his name.

In 'The Propensity Interpretation of Probability' by Karl R. Popper in the May Number, in the line headed *Postulate B*, the words 'provided *b*, *c* (and therefore *bc*) and *d* are also in *S*' have been omitted between 'and' and 'the following'; and they should be inserted, between commas.

## RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

### (a) BOOKS RECEIVED FOR REVIEW

- Anscombe, G. E. M., *An Introduction to Wittgenstein's Tractatus*, Hutchinson & Co., London, 1959, pp. 179
- Barker, S. F., *Induction and Hypothesis*, Cornell University Press, 1957, pp. xvi + 203
- Basson, A. H., *David Hume*, Penguin Books, Harmondsworth, 1958, pp. 183
- Birro, C., *The Ways of Enjoyment*, Exposition Press, New York, 1958, pp. 114, \$3.00
- Bollnow, O. F., *Die Lebensphilosophie*, Springer-Verlag, Vienna, 1958, pp. vi + 154
- Brain, Sir Russell, *The Nature of Experience*, Oxford University Press, London, 1959, pp. 73, 8s. 6d.
- Bridgman, P. W., *The Way Things Are*, Oxford University Press, London, 1959, pp. x + 333, 45s.
- Carnap, R. and Stegmüller, W., *Induktive Logik und Wahrscheinlichkeit*, Springer-Verlag, Vienna, 1959, pp. vii + 261, DM 32
- Carnap, R., *Introduction to Symbolic Logic and Its Applications*, Dover Publications, New York; London Constable & Co., 1959, pp. xiv + 241, 15s.
- Cohen, I. B., (Ed.), *Isaac Newton's Papers and Letters on Natural Philosophy*, Cambridge University Press, 1958, pp. xii + 501, 70s.
- Cooney, T., *Ultimate Desires*, Philosophical Library, New York, 1958, pp. 100
- Dewey, J., *Moral Principles in Education*, Philosophical Library, New York, 1959, pp. x. + 61, \$2.75
- Drake, H. L., *The People's Plato*, Philosophical Library, New York, 1958, pp. xxiii + 633
- Feibleman, J. K., *Inside the Great Mirror*, Martinus Nijhoff, 1958, pp. 228
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